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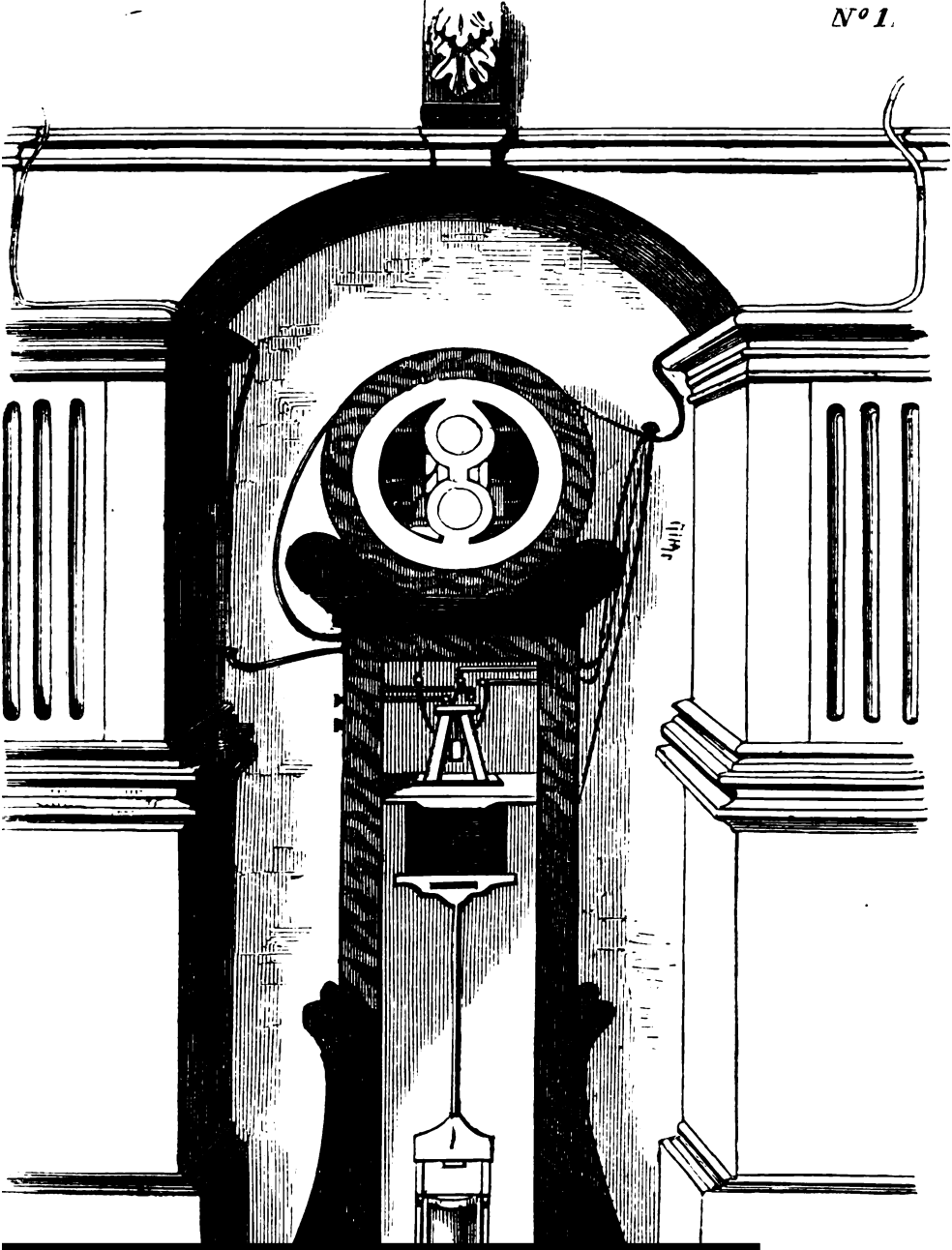

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Royal Astronomical Society

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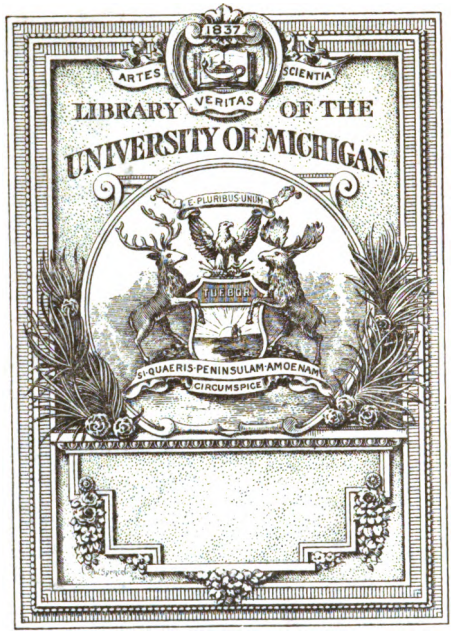

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MONTHLY NOTICES

OF THE

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ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1854, *TO JUNE* 1855.

VOL. XV.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

LONDON:

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GEORGE BARCLAY, CASTLE STREET, LEICESTER SQUARE.

1855.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *November* 10, 1854. No. 1.

G. B. AIRY, Esq. President, in the Chair.

Wadham Lock Sutton, Esq., Berkhampstead, Herts, was balloted for and duly elected a Fellow of the Society.

Vol. xxiii. of the *Memoirs* has recently been published. It contains three plates of engravings, besides a woodcut illustration accompanying one of the papers. The price of the volume is 4*s.* to Fellows of the Society and 8*s.* to the public. Vol. xiv. of the *Monthly Notices*, which contains shorter communications, abstracts of papers, lectures, &c., is given gratuitously to purchasers of the quarto volume. The two publications are supplementary to each other, and are to be considered as parts of the same series. They contain scarcely anything in common, except the Annual Report; and between them include a complete account of the proceedings of the Society during the year.

On the Construction of New Lunar Tables, and on some points in the Lunar Theory depending on the Conformation of the Moon with respect to its Centre of Gravity. By Professor Hansen.

(*Letter to the Astronomer Royal.*)

"As regards the Lunar Tables, I am now so far advanced with their construction as to be in possession of tables which represent the individual observations of the moon with a great degree of accuracy, and leave hardly any greater outstanding differences than those which are usually met with in observations of the fixed stars. After the correction of the originally assumed elliptic elements and of those coefficients of perturbation, the values of which can be ascertained *only* by observation, the following Greenwich observations were compared with the tables. In this operation there was no selection of the observations, those actually employed having been arbitrarily assumed beforehand, without knowing what might be the result of the comparison. Moreover, no further comparison of Greenwich observations with the tables has been instituted *subsequently* to the correction of the elements, so that the following results are unaccompanied by any circumstance which might tend to produce a bias in their favour.

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"The outstanding differences between the observations and the tables (theory—observation) are the following, in which α denotes the right ascension and δ the declination.

Date.		$\Delta\alpha \cos\delta$.	Remarks.		$\Delta\delta$.	Remarks.
		″			″	
1824, Mar.	8	0·0			+1·1	
	9	−2·9	...	...	−0·6	
	12	+0·2	...	...	+6·8	
	13	+2·1	...	...	−0·8	
	14	−0·9	...	...	+5·8	
	16	+2·1	..	...	−1·7	
	19	−0·6	...	...	−0·7	
Aug.	31	+4·7	...	...	+0·8	
Sept.	1	+0·5	...	...	−2·5	
	2	+0·7	...	...	−0·6	
	4	−3·2	...	...	+2·4	
	5	−1·5	...	...	...	
	6	−3·5	...	...	−1·5	
	8	−3·5			−5·2	
	8	−3·9	...	...		
	12	+5·4	...	...	+4·6	
	13	+2·7	...	...	+1·5	
	15	+0·7	...	...	+0·4	
1832, Mar.	8	−6·0	...	...	+1·5	Faint.
	9	−3·1	...	...	+2·0	Pond's Polar Distances of 1832 have been all increased by 2″·2.
	10	−3·5	...	...	−0·5	
	11	−3·4	...	...	−0·6	
	12	−0·3	...	...	+2·2	
	13	−3·0	...	...	−0·2	
	15	−4·2	...	...	−0·1	
	17	−1·6			+0·9	
	18	+2·2	F. S.	...	+2·0	
April	7	−2·3	...	...	...	
	8	−1·5	...	...	−1·5	
	9	+1·7	...	...	−0·6	
	10	−2·2	...	...	+0·6	
	11	−2·1	...	...	+1·7	
	12	+2·0	...	...	+0·9	
	13	+0·6	...	...	−0·4	
	14	−1·1	F. S.		+5·2	
	16	+2·6	...	...	−0·6	
	17	+3·4	F. S.	...	−3·0	
	20	+0·5	F. S.	...	−0·9	
July	4	−3·2	...	...	−1·2	
	8	+3·8	...	...	+0·9	
	12	+6·3	R.	...	+2·6	
	14	+5·2	R.	...	+0·7	
	16	+5·6	...	...	+5·0	
	18	+2·4	...	...	...	
	19	(+7·3)	Extr. faint.	...	−3·1	
	20	(+10·4)	R. No star of compar.		+3·1	

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Date.		$\Delta\alpha \cos\delta$.	Remarks.	$\Delta\delta$.	Remarks.
		″		″	
1838, May	2	−2·5	Very cloudy ...	+0·8	
	3	+0·1	... ...	+2·0	
	4	−1·5	... ...	−0·7	
	5	+0·4	... ...	−1·5	
	6	0·0	... ...	−1·0	
	7	+0·9	... ...	+0·3	
	8	−0·6	... ...	−2·6	
	8	...	... ...	+1·2	
	11	−2·9	... ...	−0·3	
	12	−3·7	Cloudy ...	+4·3	Cloudy.
	14	−2·2	... ...	+2·3	Faint.
	15	+1·0	... ...	+3·6	
	17	+4·4	No star of comparison.	+4·1	Very faint and flickering.
	27	0·0	... ...	−0·1	
	29	+0·6	... ...	−1·6	
	31	−0·5	... ...	−0·8	
June	2	−3·7	... ...	+1·4	
	4	−2·3	... ...	+1·4	
	6	−2·8	... ...	+3·2	North not so good as south limb.
	6	...	... ...	−0·5	
	7	−0·3	... ...	−3·3	
	8	−5·1	... ...	+0·3	
	13	−1·3	... ...	+3·9	
1843, Aug.	4	−3·4	... ..	+2·8	
	5	−2·3	... ...	−2·4	
	6	−4·2	... ...	−1·4	
	6	...	... ...	+2·4	
	7	−3·1	... ...	+1·7	
	7	...	... ...	+0·8	
	9	−2·3	... ...	+0·8	
	9	−0·6	... ...	...	
	10	0·0	... ...	+0·5	
	11	−1·7	... ...	+0·2	
	12	−2·1	... ...	+2·1	
	18	−0·5	... ...	−2·1	
	20	−1·6	... ...	...	
	22	−2·8	... ...	...	
	31	−0·5	... ..	−3·2	Merely approximative.
Sept.	2	+0·5	... ...	+0·9	
	2	...	... ...	+2·8	
	3	+0·6	... ...	+2·5	
	3	...	... ...	+2·6	
	5	−1·8*	... ...	+1·1	
	6	−3·5*	... ...	−1·1	
	6	...	* Irregularity in the	−2·3	
	7	(−8·7)*	azimuthal errors.	−2·8	
	7	...	... ...	−1·9	
	10	+2·1*	... ...	+0·8	

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Date.		$\Delta\alpha \cos\delta$.	Remarks.	$\Delta\delta$.	Remarks.
		″		″	
1843, Sept.	11	−1·1	... ...	+0·7	
	12	+1·4	... ...	+1·2	
	16	−0·6	... ...	−0·9	
	29	+0·6	... ...	−0·1	
	29	...	... ...	−2·2	
Oct.	1	−2·0	... ...	+1·1	
	4	−1·8	... ...	−1·1	
	5	−1·5	... ...	+0·9	
	9	+6·0	... ...	−1·2	
	12	...	... ...	−1·0	
	14	+1·8	... ...	−2·0	
	14	...	... ...	−1·5	
	15	+3·4	... ...	−1·7	
	16	−0·6	... ...	−2·6	
	17	+1·3	... ...	−2·2	
	18	...	... ...	−1·2	
1844, Mar.	26	−5·4	... ...	−1·6	Both observations made with great difficulty on account of clouds.
	26	...	... ...	−6·3	
	28	−2·4	... ...	−2·9	
	29	−2·1	... ...	−2·1	
	30	−0·9	... ...	−0·2	
April	1	−0·9	... ...	+1·1	
	1	...	... ...	+1·6	
	2	−1·2	... ...	+0·5	
	2	...	... ...	+2·5	
	3	0·0	... ...	−0·5	
	5	−2·4	... ...	+0·4	
	6	+0·3	... ...	−2·6	
	8	+2·3	... ...	+0·1	
	8	...	... ...	−1·4	
	9	+0·5	... ...	+2·9	
	10	+0·1	... ...	+0·7	
1850, Mar.	17	−4·5	Very faint.	...	
	18	−2·5	Very cloudy.	−6·0	Very faint.
	25	−5·3	... ...	−0·5	
	26	−4·4	... ...	+0·5	
	27	−2·0	... ...	+2·6	
	27	−1·7	... ...	−1·3	
	28	−0·7	... ...	+3·2	
	28	...	... ...	+0·8	
	29	−3·1	... ...	+3·0	
	29	...	... ...	+1·5	
April	2	+3·4	... ...	+0·8	
	2	...	... ...	+2·3	
Aug.	13	−0·9	... ...	−1·8	
	15	−0·6	... ...	+2·6	
	16	...	... ...	+0·5	
	18	−1·2	... ...	−1·8	

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Date.	$\Delta\alpha \cos\delta$.	Remarks.		$\Delta\delta$.	Remarks.
	′			′	
1850, Aug. 18	...	...	...	−0·8	
19	+0·8	...	...	−1·9	
19	...	...	...	+3·5	
20	+4·3	...	...	+1·1	
21	+5·3	...	...	+1·9	
21	...	...	...	+0·7	
22	+2·6	...	...	+0·7	
22	+2·3	...	...	+1·9	
23	+2·8	...	...	−0·3	
23	...	...	...	+0·4	
24	+3·2	...	...	+0·6	
26	−3·0	...	...	−0·5	
28	+2·7	...	...	−0·4	
29	−1·0	...	...	−0·8	
30	−2·3	...	...	−1·5	
Sept. 11	+2·5	...	...	+3·4	
12	−0·2	...	...	−0·2	
14	+0·1	...	...	+0·6	
14	...	...	...	+1·0	
18	−0·2	...	...	...	
21	+1·3	...	...	+0·8	
21	...	...	...	+0·2	
22	+1·6	...	...	−2·2	
25	−0·8	...	...	−0·5	
26	−1·4	...	...	−1·9	
27	−2·6	...	...	−3·0	

Remarks upon the observations of right ascension for the year 1832.

'In the early part of August the pivots of the transit were examined and found to be unequally, &c.

'In the reduction no notice is taken of any instrumental errors, &c.

'The personal equation between Taylor and Ellis = 0ˢ·003, which may be neglected.

"Nothing is stated respecting the observations F S and R. In these also the personal equation would seem to have been neglected.

'July 20, faint. The observer (12) could not observe the moon-culminating stars, the atmosphere being hazy.

"If we now exclude from the foregoing observations those which I have enclosed in parentheses, since they are obviously affected with errors, and calculate the sum of the squares of the outstanding deviations from the tables, we have,—

In right ascension,

Out of 139 observations the sum of the squares of the errors = 952″·1

In declination,

Out of 157 — — — = 690″·4

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Whence

The mean error of right ascension = 2′·62
— — declination = 2′·10

"If, now, in consequence of the foregoing remarks, we also exclude the observations of right ascension for the year 1832, there results in right ascension,

Out of 113 observations the sum of the squares of the errors = 676″·2;

consequently, the mean error of right ascension = 2″·44.

"These errors do not differ essentially from those which usually affect observations of the fixed stars. I may add, moreover, that it is stated, with respect to Burckhardt's Tables, that 167 observations give the sum of the squares of the errors of the tables = 4602″, which corresponds to a mean error = 5″·25.

"Besides the foregoing observations, a number of the first observations of Bradley were compared after the correction of the elements, whence there resulted the following differences between the observations and the tables :—

Date.	Δα cos δ.	Δδ.
1751.	′	′
Oct. 24	+1·4	− 1·5
25	+3·5	− 5·2
26	+1·4	+ 3·0
28	−0·3	+ 9·2
29	−0·4	− 0·3
30	−4·3	− 0·2
31	−1·4	+ 1·2
Nov. 1	−1·3	− 0·2
2	+7·3	− 4·0
2	...	+ 0·7
3	+7·8	+12·1
7	+1·6	− 7·0
8	+3·8	− 3·6
9	−4·5	+ 3·0
10	+1·9	− 1·7
12	−4·1	...
13	+3·5	...
14	−1·8	...
1752.		
Feb. 22	−3·9	+ 5·0
23	−7·5	+ 2·5
25	+0·2	+ 3·9
26	+0·2	+ 5·9
27	+1·7	+ 5·3
28	+5·0	+ 7·7
Mar. 1	−1·2	+ 3·8

Date.	Δα cos δ.	Δδ.
1752.	′	′
Mar. 2	−1·8	+ 4·0
4	−1·5	+ 3·8
6	−6·6	+ 6·1
7	−3·7	+ 3·4
7	...	+ 2·1
9	+0·7	+ 3·7
11	+3·7	...
Oct. 11	+4·6	...
12	−3·5	+11·2
13	−2·7	− 6·0
16	−6·3	+ 2·4
17	0·0	+ 1·2
18	−2·5	− 1·8
20	0·0	− 5·6
21	+7·9	−10·7
23	−0·5	− 8·4
24	−4·3	− 8·4
25	−0·2	− 7·7
27	+5·3	− 4·4
28	0·0	+ 2·4
29	+0·9	+ 1·8
30	+7·7	− 9·1
31	−2·4	− 4·7
Nov. 1	+2·6	...
2	+2·2	...

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Date. 1758.	$\Delta\alpha \cos\delta$.	$\Delta\delta$.	Date. 1758.	$\Delta\alpha \cos\delta$.	$\Delta\delta$.
	″	″		″	″
May 6	+0·2	+11·9	May 15	+6·9	+ 5·7
7	(−10·5)	+12·9	16	−0·6	+ 6·6
8	−2·3	...	22	+3·1	+ 0·4
9	−1·5	+ 5·8	23	+0·3	+ 0·1
10	−0·5	+ 6·3	24	+0·1	− 3·2
11	+1·7	− 1·1	25	+5·2	− 4·0
12	+7·3	+ 5·0	26	+3·7	− 4·4
14	+6·7	+ 4·3			

"Excluding some observations of right ascension enclosed in parentheses, we hence deduce :—

Out of 62 observations of right ascension, the sum of the squares of the errors ..... = 884″·3

Out of 57 observations of declination ..... = 1841″·0

whence,—

The mean error of a right ascension..... = 3″·78

— — — declination .... = 5″·70

Moreover, these mean errors are not greater than one might expect of such observations, when we take into consideration the observations of the fixed stars made at the same time, and bear in mind that Bradley then employed non-achromatic telescopes and the iron quadrant.

"I have also compared with the tables a series of Dorpat Observations of the moon, which furnish very beautiful results. But as there is a circumstance connected with them with respect to which I must ask the permission of our friend Struve, I will defer their publication for the present.

"I will now give the elements of the moon's orbit which I have obtained.

For the epoch,

1800, January, 0·0^{h} M.T., Greenwich.

"That is to say, for mean Greenwich noon of the 31st of December of the year 1799, there were found,—

Mean longitude of the moon	=	335°	43′ 26″·71
Longitude of the perigee	=	225	23 53·06
Longitude of the ascending node	=	33	16 31·15

"The following are the tropical mean motions in 100 Julian years :—

Of the mean longitude	=	307°	53′	39″·61
Of the perigee —	=	109	3	2·46
Of the node —	=	−134	8	59·61

with respect to which I would also remark, that, although I have reason to suppose that these motions of the perigee and the node very nearly represent the true values, still I do not give them as

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definitive results. It is besides a very difficult matter to determine these two motions accurately to a couple of seconds, since the effects of their variation upon the moon's longitude and latitude appear to be diminishing. I will now institute a comparison between these motions and those which you have found.

"From your paper, 'Corrections of the Elements of the Moon's Orbit, &c.,' the following results are deducible :—

Motion in 100 Julian Years.

		°	′	″
Of the longitude	=	307	53	21·0
Of the perigee	=	109	3	57·5
Of the node	= −	134	8	14·2

and in the paper, 'On the Eclipses of *Agathocles*, *Thales*, and *Xerxes*,' there is to be found,

		°	′	″
Motion of the perigee	=	109	3	17·4
Motion of the node	= −	134	8	54·6

"You see from this that the motions of the perigee and node last given by you agree very nearly with the values which I have found. I must, however, add this remark, that the observations comprised between the years 1820 and 1850 bear an increase of the value of the motion of perigee found by me, less than a diminution of the same value. By such a diminution the mean errors found above might be still further diminished.

"Between the motions of mean longitude found by me there exists a greater difference, but this arises from the circumstance that I have slightly altered the coefficients of the two inequalities of long period. The accurate determination of these two inequalities by theory, is the most difficult matter which presents itself in the theory of the moon's motion. I have on two occasions, and by different methods, sought to determine their values, but I have obtained results essentially different from each other. I am now again engaged with their theoretical determination by a method which I have simplified, and hope to bring the operation to a definitive close. I have also applied to my tables some coefficients which are not free from empiricisms, but which I can justify by the circumstance that they represent the ancient as well as the modern observations with great exactness ; and it may be expected that they will represent the future observations equally well. It is, moreover, an easy matter to replace these two coefficients in the tables by others, should this be deemed necessary.

"I have found the following value of the eccentricity of the lunar orbit to be employed in the table for the equation of the centre :—

$$e = 0{\cdot}05490307.$$

"I have also found, for calculating the principal term of the latitude, the following value of the inclination :—

$$I = 5^\circ\ 8'\ 40''{\cdot}21.$$

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"The coefficient of the parallactic equation I found to be,—

$$125''\cdot705;$$

an amount exceeding any which has hitherto been assigned, and which indicates a greater value of the sun's parallax than has been deduced from the observations of the transit of *Venus*. The Greenwich observations, exclusive of any others, assign the foregoing value of the parallactic inequality, and the Dorpat observations nearly the same value. I cannot, therefore, alter it. I may remark further that I have taken into consideration, not only the coefficient of the parallactic inequality itself, but also the largest of the remaining terms depending on the sun's parallax, and that the aggregate of these terms may amount to the fifth part of the coefficient of the parallactic inequality.

"The coefficients of the inequalities of the moon, depending on the [terrestrial] ellipticity, were found to have the following values:—

$$\text{In Longitude} = 7''\cdot624\,;\ \text{in Latitude} = 8''\cdot382.$$

"Add to these the terms, which I have deduced from theory, depending on the variation of the obliquity of the ecliptic, and there result in Longitude the terms,—

$$7''\cdot760 \sin(\Theta + 184^\circ\ 42') + 0''\cdot000157\,(t - 1800)\cos(\Theta + 260^\circ\ 5')\,;$$

in north latitude,—

$$8''\cdot764 \sin(v + 169^\circ\ 51') + 0''\cdot00038\cdot8\,(t - 1800)\cos(v + 278^\circ\ 39'),$$

where Θ signifies the supplement of the ascending node, and v the longitude of the moon in its orbit. The logarithm of the ratio of the moon's semidiameter, to the sine of the equatoreal horizontal parallax, was found to be as follows:—

$$\begin{aligned}\text{For the horizontal semidiameter} &= 4'\cdot750{,}484\\ \text{For the vertical semidiameter} &= 4\ 75{,}0554.\end{aligned}$$

"It is known that different telescopes assign different diameters to the stars. Now, since at Greenwich the right ascensions and declinations have been observed with different instruments, it follows that the vertical and horizontal semidiameters of the moon must be deduced independently from those data. But even in the case wherein these two polar co-ordinates have been observed with one and the same instrument—for example, the meridian circle—it becomes necessary, when the observations refer to the moon, to execute this two-fold process. For we must bear in mind that the observations are made in different ways; since the observation of declination is made by bringing the limb of the moon in contact with the horizontal wire of the instrument, or bringing it midway between two horizontal wires, while the right ascension is found by noting the times of the entrance of the moon's limb upon the vertical wires. It may very well happen, from the different modes of observing, that the hori-

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zontal semidiameter may be found to be different from the vertical, even in the case wherein both of them are equal—a supposition, however, which has no foundation in theory. Nay, I have found, while engaged in determining the moon's orbit, indications that the personal equation between two observers may be different for the sun and moon from what it is for the stars, and that it even betimes may be different for the preceding and following limbs of the moon. I have not in the mean time been able to take into account this difference in the Greenwich observations, but I feel convinced that were I to do this, the agreement between the observations and the tables would be still better than it is. The difference between the above assigned values of the horizontal and vertical semidiameter is, moreover, very small, amounting only to 0″.15. Besides the above-mentioned quantities, determined by observation, which enter into my Tables of the Moon, there are two others of which I shall speak further on, after I have given some account of the mode of constructing the Tables, and of the method employed in determining the elements.

"The perturbational coefficients employed in the construction of the tables are, with the exception of the few which can only be determined by observation, those which I have calculated from theory. They are, throughout, accurate to two hundredths of a second: many of them I consider to be still more accurate. They already exhibit sensible differences from those of Damoiseau and Plana: nay, many of them differ from the corresponding terms of these tables to the extent of 2″ and 3″.* I have not yet published the perturbations of longitude and latitude, but the expression in the tables for the moon's parallax is, with a few trifling alterations, that which I made known in No. 403 of the *Astronomische Nachrichten*. The good agreement of the tables with the observations is a proof *à posteriori* of the accuracy of my perturbational coefficients, and of the soundness of the groundwork upon which the tables rest. I have applied to the latter, not only the larger coefficients, but also the smaller ones, which, indeed, are individually very trifling, but which, in their aggregate, exercise a not inconsiderable influence on the moon's place. Moreover, I have taken into account, not only the tenths, but also the hundredths of a second, since the latter produce a sensible effect upon the sum of many terms, and if they were not included, this sum, and consequently the moon's place itself, would frequently come out sensibly erroneous by the tables. Further, the secular variations of the perturbational coefficients, which are of sensible magnitude, are considered in the tables: their effect in 100 years may amount to 3″.

"The mean motions of the perigee and node above assigned are those which have been deduced from the observations, although they admit of being rigorously found by theory. But the ex-

* The sum of the differences between Plana's coefficients and mine amounts to 52″.

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tremely accurate determination of them by theory, as their application to ancient and all other observations demands, is an operation of such magnitude, that it will be always preferable to deduce their values from the observations. There will, therefore, be found in the tables, with reference to these two quantities, an empiricism, similarly as in the case of the two inequalities of long period, of which mention has already been made.

"For the sake of simplicity, tables with double arguments are employed in entering the smaller terms of perturbation, and, as in the solar tables, these are so arranged, that, in practice, it will be necessary to perform only one interpolation upon each of them individually, the second interpolation being executed upon the sum of all the terms. The peculiar form of the arguments of the lunar perturbations imparts to these tables a greater degree of simplicity than that which characterises the solar tables, since, in most cases, they admit the employment of double arguments, whereby the extent of every such table is reduced to half the dimensions which otherwise it would be necessary to give to it.

"Since the most frequent use of lunar tables consists in this,—that they shall serve to compute the moon's place for a series of equal, or nearly equal, intervals of time, the arguments have been expressed, not in degrees, but in time; and since the next application of them consists in comparing with these results a large number of meridian observations of the moon, the unit assumed in the arguments is the mean time which elapses between two successive culminations of the moon, in the same way as in the solar tables the mean day is chosen as the unit of the argument. By this arrangement the comparison of the observations with the tables will be very much facilitated. In order to compare the observations of a month, we select any whatever of the mean solar times of the observations, and calculate the corresponding values of the arguments from the tables. With this argument, increased or diminished by the units, 1, 2, 3, &c., we calculate from the tables the corresponding places of the moon. These are available now for all times which approach very near the times of the moon's culmination, and by a process of interpolation which it is easy to execute, since the interpolation factor is small, we may then transfer the calculated places to those which refer to the mean times of the meridian observations. The accuracy of the greater part of this calculation can be verified by differences, and only in respect to a small part of them will it be necessary to employ any other test. By the aid of the same tables we may also calculate an ephemeris of the moon, giving the moon's place from day to day for the same mean times as is done in the *Nautical Almanac* and other similar works. Still in this case the labour will be considerably increased, since the arguments vary throughout each day, not by a unit, but by fractions of a unit. I would prefer for such an ephemeris tables in which the unit of the argument was a mean solar day. This mode of computation is not, by any means, so laborious.

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"For determining the corrections of the elliptic elements, &c., the following process was employed.—With the exception of the mean motions, these corrections were deduced entirely from the observations of our own time, the ancient observations being employed only in determining the mean motion. The reason for this mode of procedure is, that the more recent observations are available in much greater abundance than is necessary for our inquiry; that they are more accurate than the ancient observations, and, therefore, must lead to more accurate results than the latter; and that, even if the ancient observations were included in the investigation, the value of the final result would not be enhanced thereby, inasmuch as they are less precise than the modern observations.

"For every observation employed in this part of the inquiry the corresponding place was calculated from the tables, and this calculation was carried on to the observation itself. The right ascensions and declinations of the observed limbs were therefore calculated, and the corresponding observations subtracted from them, whereby the influence of the errors of the assumed provisional values of the elliptic elements, &c. was brought out. Now, since by one of the foregoing preliminary determinations, the provisional elements were very nearly correct, it was found that the differences between the computed results and the observations were already very small. In order to diminish these as much as possible, the coefficients of the variations of the elements to be corrected were calculated for each observation, and the equations of condition formed. The number of unknown quantities in these equations was eleven; but besides, the coefficients of the variations of the three mean motions were calculated, annexed to the equations, and retained in the subsequent calculations, with the view of being enabled thereby to ascertain the influence of a variation of the provisionally assumed mean motions upon the remaining unknown quantities.

"The equations of condition were now distributed in groups according to the magnitude of the coefficients of the different unknown quantities, and all the equations of each group added together.

"For example, in determining the mean longitude of the epoch, the eccentricity, and the longitude of the perigee of the epoch, the observations for every 30° of anomaly were grouped together, whereby twelve equations were obtained; and a similar process was employed for the other unknown quantities. Hence resulted a large number of equations, which were treated by a process similar to that which the method of least squares leads to. Namely, the above equations, which have been cited by way of example, were multiplied and added; first, with the coefficients of the variations of the mean longitude, then with the coefficients of the variations of the eccentricity; and, finally, with the coefficients of the variations of the perigee. Hence resulted three final equations; and the other equations, treated in a similar way, added eight

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more to these; so that the number of equations was the same as that of the unknown quantities, and the values of the latter were obtained by the solution of these equations. The final equations to which one is conducted by this process do *not* possess the property that the coefficients in the vertical rows are relatively equal to those in the horizontal rows, as is the case with the final equations to which the method of least squares leads; but I can prove that they must necessarily possess this property *nearly*, and consequently that the cofficients in the rows referred to must be *nearly* equal to each other, since in each combination of groups all the corresponding equations of condition were employed. Nay, if even by this process the unknown quantities were not so determined that the sum of the squares of the remaining errors will be a minimum, still the result will be that this sum will approach very near to the minimum; and this will be sufficient in the present investigation, on which the rigorous employment of the method of least squares would entail a frightful amount of labour.

"I now come to a circumstance which has not yet been taken into consideration in the lunar theory. The remarkable circumstances which distinguish the motion of rotation of the moon necessarily imply a peculiar condition of the matter of which the moon's mass is composed; and theory replies in relation thereto, that the moment of inertia with respect to the principal axis, which is nearly parallel to the radius vector, must be the least; and that the moment of inertia with respect to the axis of rotation must be the greatest, of the three moments of inertia of the mass of the moon. The next assumption relative to the figure of the moon, and which is based upon this principle, is, that it is an ellipsoid, whose greatest axis is nearly parallel to the radius vector. Observation, however, has not hitherto confirmed this assumption. If, for example, this axis was to one of the other two in the proportion of 21 to 20, then upon this ground, and in virtue of the libration, there ought to be perceptible in the moon's semidiameter a variation of 2″. I have been unable, however, to establish any variation of this kind from the observations of the moon's diameter, which are available; and if even, in fact, a variation of the axis of the moon did exist, it must be much less than that deduced from the proportion above stated. Under these circumstances, it only remains that we assume that the interior of the moon is heterogeneous; and hence results the difference of the moments of inertia, which complicates the theory of the moon's rotation. There next arises, in connexion with this circumstance, the question whether, as in the case of the planets, the centre of the moon's figure coincides with the centre of gravity, as has been always assumed, or whether the positions of these two points may not be different? If the latter condition should really exist, then there may be found many laws of the density of the interior, in virtue of which the moment of inertia relative to the radius vector which is nearly parallel to the principal axis, will be the least of the

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three, even if the figure of the moon were different from that of a sphere.

"While engaged in the determination of the elements of the moon's orbit, of which mention has been made above, I undertook to examine this question, and I have ascertained as the result of my inquiry, that the observations of the moon entirely concur in indicating a difference between the positions of the two points above referred to.

"Let α, β, γ, be the three co-ordinates of the centre of the moon relative to its centre of gravity, so that α is nearly parallel to the radius rector, β perpendicular to it in the equator, and γ perpendicular to both of these; then theory shows that α and γ may be very well determined by the observations; but β, on the contrary, can be determined only with great difficulty, or not at all. The coefficient of β is literally down to small quantities equal to the coefficient of the variation of the longitude of the perigee, and the effect of β consists, therefore in great part, only in this, that the longitude of the perigee is increased or diminished by a constant quantity. For γ the Greenwich observations give $-1''{\cdot}01$: and this quantity has been determined by itself with great certainty; only I must here remark a circumstance, which possibly may exercise an influence upon the magnitude of these quantities. It is known, that notwithstanding all the care bestowed in clearing the declinations of the stars from instrumental errors, still very frequently the declinations determined at one observatory deviate from those determined at another observatory in one direction, either north or south; and it is clear that an error of declination of this kind, should it be present, would affect the determination of γ; and the numerical value obtained for that quantity is fundamentally the aggregate of the above-mentioned co-ordinate γ and the constant error of declination.

"I must leave undecided the question, whether in the case of the Greenwich observations such an error may be supposed to exist, and leave this matter to your own judgment, since, as Director of the Greenwich Observatory, you are best acquainted with everything relative to the observations made there; but I could not refrain from a general allusion to the circumstance; nay, I even felt a desire to mention it to you.

"The co-ordinate α admits of being determined by the libration of the moon, and I have arrived at the following remarkable theorem relative to it:—

"*If the centre of gravity of the moon and the centre of its figure do not coincide, then must all the coefficients of perturbation for the mean longitude be multiplied by a constant factor, which is a function of the distance between these two centres projected upon the radius vector. If the centre of the moon be farther removed from us than the centre of gravity, then is this factor greater than unity; but if, on the contrary, the former*

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be nearer to us than the latter, the factor will be greater than unity.

"The observations enable us to decide that this factor is greater than unity, and that, consequently, the centre of the moon's figure is different from its centre of gravity, and lies nearer to us than the latter. I have given myself much trouble in applying the greatest possible certainty to this determination; I have executed it in many different ways; and on each occasion I have found, from the Dorpat as well as from the Greenwich Observations, that this factor is greater than unity, even although its absolute value, which could not be otherwise expected, exhibited a slight difference as it resulted from the various determinations. The final determination gave this factor—

$$= 1{\cdot}0001544;$$

and hence there results, among other consequences, an enlargement of the coefficient of the evection equal to $0''{\cdot}69$, and it is easily recognised that the enlargement of the sum of all the perturbations may exceed $1''$.

"Esteemed Friend and Colleague! you have found by your discussion of the Greenwich Observations, extending from 1750 to 1830, that the principal coefficients of the lunar perturbations ought to be increased. You have found the increase of Plana's coefficient of the evection to be $= 1''{\cdot}28$, and the increase of his coefficient of the variation to be $= 0''{\cdot}68$; now, since the coefficient of the evection is almost double that of the variation, these enlargements seem to indicate the presence of a constant error.

"The increase of these coefficients which you have found is in each case greater than that which I have deduced, only I must here remark that Plana's coefficient of the evection is $0''{\cdot}34$ less than mine; and this circumstance leads to a closer agreement of our results. I would remark further, that you have found that Plana's coefficient of the annual equation ought to be increased by $1''{\cdot}07$, but your coefficient is too small by $1''{\cdot}1$, and therefore the extent of the increase will be considerably diminished.

"Permit me, in conclusion, to offer a few remarks on the foregoing explanation of the enlargement of the coefficients of the lunar perturbations. From the above-cited value of the factor it follows, that the centre of the moon's figure lies about 59,000 mètres; that is, about 8 geographical miles (reckoning 15 miles to a degree of the equator) nearer to us than the centre of gravity; and hence it follows, that between the two hemispheres of the moon, of which the one is directed towards the earth and the other is turned away in the opposite direction, there must exist a considerable difference with respect to level, climate, and all other circumstances depending thereon. Since the strata of homogeneous density must arrange themselves nearly in relation to the centre of gravity, it follows, if we suppose the figure of

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the moon to be a sphere, that the centre of the visible disk of the moon lies about 59,000 mètres above the mean level, and the centre of the opposite hemisphere almost as much under the same level: I say 'almost,' since if, as we here must assume, the opposite hemisphere of the moon is more dense than the hemisphere turned towards us, it necessarily follows that the mean level of the former will be somewhat depressed, and the mean level of the latter somewhat elevated. If we suppose the moon to be an ellipsoid, the elongation of which lies in the direction of the earth, then the hemisphere of the moon which is next the earth will rise a little more above the mean level, and the opposite hemisphere will sink a little more beneath it. Nay, we may consider it as not impossible that the surface of the opposite hemisphere of the moon wholly or partially accommodates itself to one and the same level, in a similar way as we find to be the case with the earth.

"We need not, then, under these circumstances, wonder that the moon, when viewed from the earth, appears to be a barren region deprived of an atmosphere, and of all animal and vegetable life, since, if there existed upon the earth a mountain proportionally high, and, consequently, having an elevation of 216,000 mètres, or 29 geographical miles, there would not be recognisable upon its summit the slightest trace of an atmosphere, or of anything depending thereon. We must not, however, conclude that, on the opposite hemisphere of the moon, the same relations exist; but rather, we should expect, in consequence of the distance of the centre of figure from the centre of gravity, that an atmosphere and animal and vegetable life may there find place. Nearly at the moon's limbs the mean level must exist; consequently, we might reasonably expect to discover there some trace of an atmosphere.

"If we now inquire into the cause of this condition of the moon, I hold it to be not impossible that volcanic and other similar forces in the interior of that body may have met with far less opposition in one of the hemispheres than in the other, and may therefore have effected much greater upheavings of the surface in the former than in the latter. I am also disposed to think that the phenomena termed 'Rills,' which are perceptible on the moon's surface, and respecting which selenographers do not seem to have yet, upon the whole, arrived at any satisfactory conclusion, are rents or splits, which have been occasioned by these enormous upheavings. I submit to the judgment of astronomers these considerations, which, strictly speaking, do not belong to the theory to which this letter is devoted, but which originate in the distance of the centre of gravity of the moon from its centre of figure.

"The theory of the moon's figure, which leads to the theorem above cited, as well as to several other conclusions, has been developed by me in a Memoir which I have the honour of herewith communicating to the Royal Astronomical Society.

"*Gotha*, 1854, *Nov.* 3."

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Account of the Re-mounting of his Observatory, with some Observations of the Satellite of Neptune. By William Lassell, Esq.

(*Letter to the Editor.*)

"Previous to my sending you a very small result of my astronomical labours this autumn, I may state that owing to the encroachment of buildings around my former residence of Starfield, in the vicinity of Liverpool, I have been driven two miles further into the country in the same direction, eastward of the town; and have found at least in this locality (which I consider for the present to be situated in latitude 53° 25′ 28″, and longitude west from Greenwich $11^m\ 38^s.7$), a less smoky atmosphere, and a more rural expanse of prospect; whether a less cloudy or less disturbed atmosphere, some experience will be required to determine.

"During two or three months of the summer I was closely occupied in the removal of the two circular buildings and their domes, respectively 15 and 30 feet in diameter. The smaller dome was removed entire, on a four-wheel spring wagon. The larger was separated into four parts, as an orange is quartered, by running a saw down, so as to remove the boarding between two adjacent ribs 90° apart. Two of these quadrants, one partly placed within the other, were loaded, likewise on the same description of carriage, in a vertical position, nearly as when forming a part of the dome; and thus the whole in three journeys were safely brought to their new situation.

"Before the separation of the dome into quarters, iron plates, secured by screw-bolts, were attached across the intended sections of the base ring and of the semicircular arches which contain the grooves in which the shutters slide; and thus, when re-erected here, the replacement of the plates and bolts insured the exact juxta-position of the several quadrants. This plan has been attended with complete success, and the small portion of the removed boarding having been replaced and covered with new canvass, and the whole repainted, I believe no visible trace of its removal now remains.

"I embraced the opportunity thus afforded of improving the mounting of the dome, in respect of facility of motion, by turning a slight groove in the middle of the rim or circumference of the wheels, and introducing above and below a light curved railway bar. This has sensibly reduced the friction; and I now, without any appliance of mechanical power, can walk round with the dome, as it were, in my hand; thus carrying round with me a weight of 2400 lbs. without any inconvenient exercise of force.

"The stone piers and woodwork of both buildings, including the floors, wall-plates, and framing of lateral pulleys, were all removed with very slight derangement, and replaced in the two new circular buildings erected of precisely the same dimensions,—the very bricks of the old buildings being all removed for the

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purpose. The durability of this construction of dome has been in some degree tested by this removal; for, though the smaller dome has now been erected about fifteen years, no sign of decay was discovered during its translation; even the covering of canvass requiring no repair, whilst its freedom from strain or warping during the process has been such that the quadrantal shutter slides as easily and fits as well as when in its original position.

"Since the remounting of my observatory I have found opportunity for but few observations hitherto, but having been fortunate enough to obtain a remarkably good set of measures of the satellite of *Neptune* in position and distance, I request permission to put them on record by thus communicating them to the Society.

1854, August 29th, Tuesday.

Measures of Position, with powers 366 and 614.

	°	′
	202	56
	203	4
	202	1
	201	56
	201	46
	202	32
	200	0
	201	40
Mean	201	59
Zero	253	45
	51	46
	38	14

= angle of position at $12^h\ 20^m$ Gr. M.T. Mean epoch of measures.

Measures of Distance; power 614.

	0·842 revolutions.
	790
	832
	829
	864
	866
	842
	0·837
Mean	0·838′
Zero	−0·016
Hence	0·822 = 17″·86

= distance at $13^h\ 5^m$ Gr. M.T. Mean epoch of measures.

"*Bradstones, West Derby, near Liverpool, 9th November*, 1854."

NEW PLANET.

Letter from M. Chacornac to Mr. Hind, dated Imperial Observatory of Paris, October 30th, 1854.

"I have the honour to inform you that in the night of the 28th–29th of October, at $2^h\ 10^m$ A.M., I met with a new planet; the positions of which are as follows:—

	Paris M.T. h m s	R.A. h m s	Decl. ° ′ ″	
Oct. 28	14 17 24·3	2 33 55·16	+16 58 43·7	Equatoreal.
29	12 1 14·5	2 33 8·97	+16 55 56·9	Meridian.

"This planet is the 33d; M. Goldschmidt will have announced to you to-day his discovery of the 32d."

This planet has been named *Polyhymnia*.—Ed.

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NEW PLANET.

Extracts of Letters from Mr. Hermann Goldschmidt to Mr. Hind.

"*Paris, October 30th*, 1854.

"I have the pleasure to inform you of the discovery I made in the evening of the 26th inst. of a new (32) planet. It appears as a star of the 11th magnitude. I give you the approximate positions for facilitating its research, and also those of the Imperial Observatory.

Approximate Graphical Positions.

	Paris M.T. h m	R.A. h m s	Decl. ° ′
Oct. 26	10 0	2 26 13	+15 5
27	8 30	25 23	15 0
	17 20	2 25 6	+14 58

Positions of the Observatory.

	Paris M.T.		
Oct. 28	$13^h\ 18^m\ 11^s{\cdot}2$	R.A. = R.A. $* - 6^m\ 33^s{\cdot}94$	
		δ = δ $* + 3'\ 34''{\cdot}8$	
	Approx. Place of Star	R.A. = $2^h\ 30^m\ 55^s$	$\delta = +14°\ 51'{\cdot}5$
Oct. 29	$11^h\ 51^m\ 52^s{\cdot}0$	R.A. = R.A. $* + 7^m\ 1^s\ 80$	
		δ = δ $* - 6'\ 0''{\cdot}2$	
	Approx. Place of Star,	= R.A. = $2^h\ 16^m\ 33^s$	$\delta = +14°\ 44'$
Oct. 29	$11^h\ 51^m\ 40^s{\cdot}1$	R.A. = $2^h\ 23^m\ 33^s{\cdot}03$	Meridian.

"*Paris, November 3d*, 1854.

"I certainly owe the discovery to your beautiful ecliptical charts, which have been in my possession a few months.

"Mr. Le Verrier selected the name *Pomona* for this 32d planet."

Notes on Chinese Astronomy. By John Williams, Esq.

"In presenting the Society with some illustrations of Chinese astronomy, I feel myself called upon to give some account and explanation of them.

"The first is a complete Chinese celestial atlas, in thirty-two maps. These, with one exception, have been traced from a work forming part of the Chinese Library of the late Dr. Morrison, now in the possession of University College, Gower Street, and which, by the kindness of Professor De Morgan, I have had an opportunity of examining during the past summer. This work is in 150 volumes; it is entitled 'Illustrations of the three great Principles of Nature.' These, according to the Chinese, are heaven, earth, and man. The first part of this work is devoted to astronomy. The maps have been carefully compared with those in other Chinese astro-

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nomical works which have come under my observation, and also with the Chinese star-maps in the possession of the Society, and the positions of the several asterisms, ascertained as accurately as possible, by collation with European maps, and by comparing them with the 'Names of Stars and Constellations' in Dr. Morrison's Chinese Dictionary, collected for that work by John Reeves, Esq. F.R.A.S. They have likewise been compared with a catalogue in a work entitled 'Observationes Mathematicæ et Physicæ in India et China factæ a Patre Francisco Noel. Soc. Jes.,' 4to., Prague, 1710, which I have now the pleasure of presenting to the Society, and also in many instances with the heavens themselves. The results are given in the margin of each map, where will be found the Chinese name of the star or constellation in the original character, and also in English letters, with a reference to the corresponding stars in European maps. The maps are arranged according to the Chinese custom, their order being from right to left.

"The second consists of a pair of celestial planispheres of large size, published in this country about a century ago, and formerly belonging to the Mathematical Society of Spitalfields. On these I have traced in red lines the corresponding Chinese asterisms, with their names in the original characters and in English letters. The Chinese astronomers divide the visible heavens as seen in their country into thirty-one parts. Twenty-eight of these may be considered as answering to our zodiac, or, perhaps, rather to the twenty-eight lunar mansions of the astrologers. The other three are called by a word signifying a wall or enclosure, and which also implies an enclosed space. The first of these consists of the northern circumpolar stars. It comprises stars in *Ursa Minor*, *Draco*, *Cepheus*, *Cassiopeia*, *Ursa Major*, *Camelopardalis*, *Boötes*, and *Auriga*. The second comprises *Hercules*, part of *Ophiuchus*, *Serpens*, *Corona Borealis*, and part of *Boötes*. The greater portion of this is bounded by a line drawn from *β Herculis* through the head and body of *Serpens*, and the lower part of *Ophiuchus*, thence through the remainder of *Serpens* to *δ Herculis*. The third division is bounded principally by stars in *Virgo* and *Leo*, and consists of part of the constellations *Virgo*, *Leo*, *Leo Minor*, *Boötes*, *Ursa Major*, and the whole of *Coma Berenices*; I have distinguished the names of these three divisions by inscribing them within an orange-coloured line.

"The remaining twenty-eight divisions comprise the rest of the heavens. They are exceedingly unequal in extent, some containing many degrees of right ascension, others very few. They take their names from a group of stars generally situated in the centre of each division. The surrounding groups have various names, and in this respect answer in some measure to our constellations. They are, however, far more numerous. The figures of these groups, or constellations as they may be called, have no resemblance whatever to those given in European maps, the stars of each asterism being connected merely by straight lines.

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"It is, however, considered that each of these twenty-eight divisions or constellations has a particular animal on earth which corresponds to it, and also that it is under the influence of one of the seven heavenly bodies, which are the Sun, the Moon, and the five planets, *Mercury*, *Venus*, *Mars*, *Jupiter*, and *Saturn*. Some authors say that twenty-eight celebrated generals, who flourished during the Han dynasty, *i. e.* from B.C. 206 to A.D. 223, were, after death, changed into these twenty-eight constellations; thus affording a curious correspondence with the ancient astronomy of the western world, in which celebrated characters are fabled to have been changed after death into constellations. The Chinese also parcel out these twenty-eight divisions into four portions of seven each. According to them, the first seven are to be reckoned from the south towards the east; the stars from which they take their names are situated in *Virgo*, *Libra*, *Scorpio*, and *Sagittarius*. The next seven are between the east and north, consisting of stars in *Sagittarius*, *Capricornus*, *Aquarius*, *Pegasus*, and *Andromeda*. Seven are situated between the north and west, comprising stars in *Andromeda*, *Aries*, *Musca*, *Taurus*, and *Orion;* and the remaining seven, between the west and south, being stars in *Gemini*, *Cancer*, *Hydra*, *Crater*, and *Corvus*. It must, however, be observed, that these divisions are not only very unequal in extent from east to west, but also from north to south, and that they differ exceedingly in the comparative number of stars in them. Thus the division *Ke* consists only of four stars in *Sagittarius* and *Telescopium*, forming the central group giving the name, and three stars in *Ara*, while the division *Tsing* consists of the whole of *Gemini*, having to the north stars in *Perseus*, and to the south stars in *Canis Minor*, *Monoceros*, *Canis Major*, *Argo*, and *Columba*. Again, the division *Tsuy* comprises the stars in the head of *Orion*, and a few to the north in *Auriga;* and the succeeding one, *Tsan*, consists of the remaining bright stars in *Orion* and stars in *Lepus*, *Columba*, and *Eridanus*. I instance these as examples of the extreme inequality of the several divisions.

"In these planispheres I have distinguished the names of the twenty-eight divisions by inscribing them within blue lines.

"In addition to the thirty-one divisions of the visible heavens, I have inserted, both in the planispheres and in the atlas, the southern stars not visible in China. These are taken from one of the star-maps in the possession of the Society; and I must here observe that, however ancient and original the northern asterisms of the Chinese may be considered, there is internal evidence to prove that these southern constellations, at least, have been derived from European sources. Thus the constellation *Phœnix* is called in Chinese 'Fire Bird;' *Dorado*, 'Gold Fish;' *Apis*, 'Honey Bee;' and so on; and the stars of each constellation are also confined to the figures, as given in our maps,—a circumstance which does not occur in the more northern asterisms.

"In tracing upon these planispheres the Chinese asterisms, every care has been taken to render them as accurate as possible. The

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chief authorities from which they were taken were the Chinese star-maps in the possession of the Society. These consist of

"Two planispheres, giving the northern and southern hemispheres. Presented by Capt. Sir E. Home, Bart. R.N.

"The northern and southern hemispheres, in one sheet, with a list of the constellations and other particulars.

"An impression from the wooden block in the possession of the Society, showing the stars visible in China.

"Two planispheres in manuscript, giving the northern and southern hemispheres. On these the figures of the asterisms are very neatly drawn, and their names very distinctly written. The number of the stars in each constellation is also given, and each star has its corresponding number.

"A planisphere, showing the stars visible in China, and in a smaller circle, the southern stars not visible in the north of that country.

"The whole of these were presented by Mr. Reeves.

"The first three of these are evidently from European authorities, as divisions and numerals appear on them, which could not be derived from any other source. They are also rather meagre in names, often in many parts ill printed, and much confused in the details.

"The last two are more distinct, and appear also to be more original; and these formed the basis of the work I had undertaken, and afforded me very valuable information and assistance in it. Every group of stars in these was carefully examined; the relative position of each star determined as accurately as possible, and verified by comparison with Mr. Reeves' valuable contribution to Dr. Morrison's Dictionary, to which I have before alluded. They were also compared with the catalogue in Father Noel's work, also mentioned before. This I found of great assistance in doubtful cases. The other planispheres were also referred to occasionally; and although I cannot be satisfied that in every instance I have attained perfect accuracy, still I may confidently say that the positions assigned are, generally speaking, sufficiently correct for all ordinary purposes.

"The work by Father Noel, to which I have more than once alluded, is the third contribution to this subject. Among other things, it contains an account and explanation of the Chinese cycle of 60 years, and of the mode of its formation. It also contains a catalogue of stars, or rather asterisms, arranged according to our constellations, with the Chinese names in English characters, spelt according to the Portuguese method.

"In one part of the work the author regrets being compelled to omit the original Chinese characters for the names, &c., as he had no means of procuring the requisite types. This defect I have supplied in manuscript; and the necessity of it will be sufficiently apparent when I inform you that the Chinese vocables, being very limited, each has, in consequence, to express the sense

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of many characters, whose form and meaning are totally different. Thus the characters answering to the sound of our letter E are at the least not fewer than 267, all of them distinct in their meaning, and varying exceedingly in their forms. It, consequently, follows that any attempt to ascertain the meaning of the simple vocable E occurring in a Chinese sentence written in European letters, would be almost, if not absolutely, unavailing without the original character intended to be expressed by that sound.

"In conclusion, I have only to express my hope that the pains I have bestowed on this subject will not be entirely without their use to such persons as may hereafter have occasion to investigate Chinese astronomy. At all events, they will save a vast deal of preliminary labour, which would, of necessity, be required before the party could be in a position to proceed satisfactorily with the object he might have in view. I may also state that it is possible I may, on some future occasion (should it be agreeable to the Society), enter more at large upon Chinese astronomy; and by giving an analysis of a work on this subject, published by themselves, which is in my possession, enable you to form some idea of the value of the astronomical attainments of that singular people.

"*November* 1854."

A Method of finding the Greenwich Mean Time at Sea by Computing the Moon's true Right Ascension from the observed Lunar Distance. By M. C. Rümker.

(*Communicated by the Astronomer Royal.*)

"When the observed distance of the star from the moon is small and the difference of their declination great, the error committed in determining the Greenwich mean time, by assuming the change of time proportional to that of the distance, can scarcely be compensated by the method of taking the difference of successive proportional logarithms, whereas the change of the moon's right ascension will be found more regular. I propose, therefore, to convert the observed sun's or star's distance from the moon into her true right ascension, whence the Greenwich mean time may be found by interpolation in the *Nautical Almanac* with so much the more ease and precision, as the moon's place is given there for every hour. Thus, moreover, the expense of the calculation and printing of the distances might be saved, and the volume of the *Almanac* become less voluminous.

"It remains, therefore, to be shown that the moon's true right ascension may be found with no less accuracy, and by a not more complicated calculation than the true distance.

"In order to determine the longitude, the mean time on board must be ascertained, whence the hour-angle of the star is known, and by subtracting this angle from their difference of right ascension, or adding it thereto, we obtain the hour-angle of the moon. Denoting now by α the difference of the true right ascensions of

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the sun or star and the moon, by D, their apparent distance, by p, p', their true, and by P, P', their apparent polar distance; by t and $t' = \alpha \mp t$, their hour-angles; by z, z', their zenith distances; by s, s', their parallactic angles; by $\varrho - \pi$, or $\pi' - \varrho'$, the differences of refraction and parallax in altitude; by $d\alpha$ and dp, the effect thereof on the right ascension and polar distance; by φ', the geocentric latitude of the place of observation, lest at sea it might be judged sufficient to assume the geographic latitude for it; then the strict formula, which answers to both bodies by substituting p' for p, s' for s, and $\pi' - \varrho'$ for $\varrho - \pi$, when applied to the moon, is

$$\frac{\sin t \cos \varphi'}{\sin z} = \sin s, \quad \tan(\varrho - \pi) \cos s = \tan n, \quad \frac{\sin n \tan s}{\sin(p \mp n)} = \tan d\alpha,$$

$$\tan \frac{(p \mp n)}{\cos d\alpha} = \tan \mathrm{P}.$$

"The lower sign applies to the moon; but if s is obtuse the signs must be changed.

"Generally it will be found sufficient to make

$$(\varrho - \pi) \cos s' = dp \text{ and } \frac{(\varrho - \pi) \sin s'}{\frac{\sin(p + \mathrm{P})}{z}} = d\alpha,$$

where the following practical rule may serve as guide or confirmation.

"When the parallactic angle s' is acute, then is

True Polar Distance { p of the sun or star $- dp$ = apparent polar distance P
p' of the moon $+ dp'$ = apparent polar distance P'.

"When the parallactic angle is obtuse the signs must be changed. Should any doubt exist whether s is acute or obtuse, it must be found by another, to be shown hereafter. Then is, when A denotes the apparent difference of right ascensions,

$$\sqrt{\frac{\sin \tfrac{1}{2}(\mathrm{D} + \mathrm{P} - \mathrm{P}') \cos \tfrac{1}{2}(\mathrm{D} + \mathrm{P} - \mathrm{P}')}{\sin \mathrm{P} \sin \mathrm{P}'}} = \sin \tfrac{1}{2} \mathrm{A}.$$

"If, now, the sun or star, and the moon, were observed on different sides of the meridian, then is

$$\mathrm{A} + d\alpha - d\alpha = \alpha;$$

but if both bodies were observed on the same side, then is,

When the sun or star was highest $\mathrm{A} - d\alpha - d\alpha'$
moon was highest $\mathrm{A} + d\alpha + d\alpha$,

and the sun's or star's right ascension $\pm \alpha$ = moon's right ascension.

"In the following example the reduction to the geocentric zenith has been neglected.

"Sept. 30, 1857, at $11^h\ 15^m\ 54^s$ mean time P.M., being then in $16^\circ\ 29'$ south latitude and $92^\circ\ 2'$ west longitude, a set of distances

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of *Jupiter* from the moon's remote limb, with the corresponding altitudes, were observed from 22½ feet elevation above the level of the sea, the height of the barometer being 29in·6; height of thermometer 14° Réaumur. The chronometer was 2m 9s slow of Greenwich mean time. The means gave

Chronometer.	Jupiter, East.	Moon.	Dist. of remote Limb.
h m s	° ′ ″	° ′ ″	° ′ ″
5 21 54 A.M., 31 Sept., Gr.	36 32 0	68 53 37	69 26 18·2
+ 2 9	Dip −4 42	− 4 42	Ang. Sem. − 16 16·7
5 24 3 Gr. M.T.	Ap. Alt. 36 27 18	68 48 55	D 69 10 1·5
	ϱ − π ± 1 15·2	Moon's Ang. Sem. 16 17	
	True Alt. 36 26 3	Ap. Alt. 69 5 12	
	Z = True Zen. Dist. 53 33 57	π − ϱ + 20 34·7	
		True Alt. 69 25 46·7	
		Z′ 20 34 13	

"For the Greenwich mean time we find in the *Nautical Almanac* the sun's mean R.A. = 12h 39m 34s·6; the moon's Declin. − 11° 8′ 45″; eq. hor. par. 58′ 37″·7; Red. − 1; the moon's semid. 16′ 0″·7; *Jupiter's* R.A. 2h 51m 8s·6; Decl. + 14° 59′ 0″; hor. par. 2″·0; the moon's app. R.A. 22h 33m 34s·6.

"The hour-angle *t* of *Jupiter*, computed from his altitude results, 2h 55m 40s·1, which, being subtracted from their difference of R.A. 4h 17m 34s·0, leaves for the moon's hour-angle *t′* = 1h 21m 53s·9. The parallactic angle of *Jupiter* is found, *s* = 55° 45′ 34″, *dα* = 64″·35, P = 104° 58′ 17″·5. For the moon we obtain *s′* = 72° 40′ 26″, *dα* = 20′ 0″·9, P′ = 78° 57′ 22″·8,

	h m s
A	64 42 18
dα	+ 1 4·35
dα′	− 20 1·1
	64 23 21·25
α in time	4 17 33·41
Jupiter's R.A.	2 51 8·60
Comput. R.A. of Moon	22 33 35·19
Moon's R.A. at 17h	22 32 44·37
18h	22 34 51·12

	h m s		
	3600		Log 3·556303
Diff.	0 50·82		log 1·706035
Diff.	2 6·75		log C 7·807052
Green. M.T.	17 24 3·4		3·159390
M.T. on board	11 15 54·0		
Long. in Time	6 8 9·4	= 9° 2′ 21″ West.	

"It is true that an error in the polar distances will affect the calculation of the difference of right ascension, under the above-mentioned supposition of a small distance and a proportionally great difference of declination, more than under more favourable circumstances. But it is equally evident that a *tabular* error of declination will influence, in the same measure, the true distance in the *Nautical Almanac*, which, in that case, will be affected with nearly the whole error of the declination, whereas the influence of

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any faults in the refraction and parallax, arising from erroneously observed altitudes, will remain the same, whether $\varrho - \pi$ is divided in its effects upon the distance, or upon right ascension and polar distance. Indeed, we may say the same as to the clearing of the apparent distance, when the difference of azimuths is small, and the difference of altitudes great, where then the true distance will be affected with nearly the entire errors of refraction and parallax in altitude. Hence it follows that, with respect to accuracy, there is no other difference between the two methods, than that the interpolation may be achieved more correctly by means of the right ascensions given for every hour, and that the distances may be spared. Nor offers, when the altitudes have been observed, the method proposed by me any advantages with respect to the numerical calculation. But setting aside that little dependency can be placed upon the altitudes of stars observed above the sea-horizon, on account of the variable terrestrial refraction, and that this horizon can scarcely be distinguished at night, it should be remembered that even in the day-time the altitude of the moon's crescent, when high and faint, is difficult to be observed, and that when the line joining her cusps is perpendicular to the horizon, her real upper and lower limbs are invisible; and as it may hardly be expected that the more disinterested assistants, with their not always well-adjusted quadrants, will be sufficiently attentive to the given signal, it must be owned that, if the chronometer, or approximate Greenwich time, is but known within moderate limits, the declination of the heavenly bodies is a more certain argument than their altitudes, and that computed altitudes of the moon and stars are generally better than the observed altitudes. Not to expatiate, I will only remark, that if the latitude is sufficiently known, the mean time on board may always be obtained by altitudes of the sun observed previous or subsequent to the distance, and almost independent of the latitude, twice a-day when the sun is in the prime vertical; so that, with the assistance of a good time-piece and the log, the mean time may be known at all times of the day, and be controlled by observations on the next day. It will, therefore, be advisable, when a lunar distance has been taken, at least to confirm or to correct the observed altitudes by computation, which offers the opportunity of introducing the geocentric latitude; and in that case, the method of finding the Greenwich time, by the right ascension of the moon, requires less logarithms than a correct method of clearing the distance.

"The parallactic angles and the altitudes may be found by any other formula, but I propose the following:—

$$\cot \varphi' \cos t = \tan m, \quad \frac{\cot t \sin(p \mp m)}{\sin m} = \cot s', \quad \cot(p \mp m) \cos s' = \tan h$$

where φ' is the geocentric latitude, m an auxiliary arc, h the altitude; the lower sign must be used when the hour-angle is obtuse.

"May 29th, 1857, in latitude 53° 33′ North, and longitude

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9° 58′ 30″ East, of distances of the sun's and moon's limbs were observed, corresponding to a watch regulated to mean time. Barometer 29^{in}·9 Thermometer 15° Réaumur.

Mean of Times.	Mean of Distances.
h m s	h m s
4 39 54 P.M.	79 29 36·5
	15 36·5
	15 47·5
Appt. Dist. D	80 1 0·5

"From the *Nautical Almanac* we obtain sun's mean R.A. 4^h 28^m 29^s·49 ; sun's true R.A., 4^h 25^m 36^s·623 ; equat. of time, + 2^m 52^s·9 ; sun's Decl., + 21° 41′ 14″·3 ; sun's semid., 15′ 48″·5 − 1′ contract ; moon's R.A., 10^h 4^m 28^s·45 ; moon's Decl., + 15° 2′ 35″·8 ; moon's semid. 15′ 24″·5 + augmen. 12″ ; moon's eq. hor. par. = 56′ 24″·8, Red. for spheroid = 7″·2 ; geocentric latitude $\varphi' =$ 53° 22′ 10″, $t = 70° 41′ 43″·5$, $t' = 14° 1′ 15″$.

"Thence we find for the sun $\iota = 39° 55′ 41″$, $Z = 61° 19′ 29″ = \zeta - \pi$ by second approx. = 1′ 44″·3 ; $d\alpha = 1′ 12″·05$; $P = 68° 17′ 26″$.

"For the moon $\iota' = 13° 1′ 40″$; $Z' = 39° 53′ 8″·5$; $\pi' - \zeta' = 35′ 45″·5$; $d\alpha' = 8′ 19″·48$; $P' = 75° 32′ 14″·3$; A 84° 50′ 9″·0; $\alpha = 84° 43′ 1″·57$, in time 5^h 38^m 52^s·105, which, being added to the sun's true R.A. 4^h 25^m 36^s·623, gives for the moon's true R.A. 10^h 4^m 28^s·728, whence the longitude will be found 9° 58′·24."

On the Connexion between the Sun's Motion and the Variations of Terrestrial Magnetism. By Professor Secchi.

(*Extract of a Letter to Capt. Manners, R.N.*)

"Colonel Sabine, as you know, has published and discussed with admirable sagacity an immense mass of observations of the phenomena of terrestrial magnetism, and in the last volume of the the Toronto Observations has shown that in the tropical regions the opposition of oscillation of the needle follows so strictly the change of the sun's declination, that the change of the former can be traced to the very day of the equinox. This consequence, although apparent from the curves of the tropical countries, was not so clearly perceived in the countries more distant from the tropics. But it was evident, that if the law was true, it would become visible everywhere, provided a suitable mode of analysis should be applied. I thought, therefore, that the want of its being apparent there, was due to the kind of graphical analysis used until now, which consists in referring the monthly curves, either to the absolute mean of the year, or to the mean of the month itself. I undertook, therefore, to refer the monthly curves to the mean annual hourly curve of the year. The reason of doing so is this : the effect of diurnal variation at any hour (as far as it depends on the sun) is the complex effect of its hour-angle and of declination. The mean annual hourly curve, as deduced from the observations of the whole year, is a residual function independent of the declination which acts in opposite directions in

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the two halves of it. Therefore, if from any mean monthly curve we subtract the mean hourly curve of the year, the residual quantity so obtained will be a function of the declination of the sun. I have, therefore, executed these operations (which are done very easily, using the tables and plates of Colonel Sabine exhibiting the monthly ranges of the needle variation in declination), and the result has been a good deal more satisfactory than I ever expected. The results for the four colonial observatories are contained in the figures here enclosed, and a simple glance at them shows that for opposite declinations of the sun their curvatures are directed in opposite parts, and that their change can be traced to the very day of the equinox. Even some irregularities which may puzzle in the figures of Colonel Sabine for the tropical regions, have their explanation here. Thus, for instance, at St. Helena the curve of June has a descending flexure which is wanting in that of January. Now the differential curves of the declination show that this branch disappears from the interference of a similar branch, but opposed in the two months of opposite declination. We can generally state, that the mean curve of the declinometer at any country of the globe results from the interference of the two curves, one depending only on the diurnal period, and the other on the declination of the sun: the first is constant in its direction through all the year, and the second changes according to the position of the sun relatively to the equator. The inspection of these curves shows a general feature of the solar periodical influence: this is, that the maximum and minimum differ in time very nearly six hours; and their phases depend on the passage of the sun through the magnetic meridian of the place, so that an acceleration occurs in those places where the north declination is greater. For the southern hemisphere the complete antagonism of forces requires a contrary law.

"These facts seem to strengthen the opinion that the solar action is a direct one on the needle. To verify this hypothesis, I have subjected it to calculation in this way: I consider the needle, according to the Anpérian theory, as an element of galvanic current circulating during the day in a circle, which is the parallel that it describes during the diurnal rotation of the earth; and applying to this hypothesis the formula given by Savary for the action of a magnet on an element of current circulating in a circle, neglecting the terms of the order higher than the first and those depending on the parallax of the sun, I obtain the three following expressions for the components of the solar action, relatively to the horizon as a fundamental plane of co-ordinates:

$$X_0 = \frac{k\,l\,R}{r^2} \sin\delta \sin\omega,$$

$$Y_0 = \frac{k\,l\,R}{r^2} \cos(L+\delta)\cos\omega,$$

$$Z_0 = \frac{k\,l\,R}{r^2} \sin(L+\delta)\cos\omega,$$

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and the resultant,

$$S = \frac{k l R}{r^2} \sqrt{1 - \cos^2 \delta \sin^2 \omega}.$$

Where X_0 is the component directed perpendicularly to the meridian along the line east-west; Y_0 that directed along the line south-north, and Z_0 is vertical; k is a constant co-efficient dependent on the intensity of solar force; l, the length and magnetical moment of the needle; r, the distance of the sun from the centre of the circle; R, the radius of the parallel described by the needle; L, the latitude of the place; δ, the declination of the sun; ω, its hour-angle. If now we call Δ the deviation of the needle from its mean position, and P the resultant of those forces which act in that plane in which the needle is only moveable, we will have

$$\Delta = \frac{P}{\tau} \sin h + 2 \frac{P^2}{\tau^2} \sin 2h + \ldots .$$

where τ is the component of the force of terrestrial magnetism or gravity, which acts in the plane of P, and h the angle between P and the mean position of the needle; and also here we will retain only the first term.

"To find the different laws of motion for the three well-known magnetical instruments, let us suppose first a place where the declination of the needle $= o$: in this case the magnetic and astronomical meridians coincide; and for the declinometer we shall have $P = S \cos b$, b being the altitude of the sun, and calling a its azimuth, it will be

$$\Delta = \frac{k l R}{\tau r^2} \sqrt{1 - \cos^2 \delta \sin^2 \omega} \cos b \sin a,$$

and since

$$\cos x = \cos b \sin a = \sin \omega \cos \delta,$$

we have

$$\Delta = \frac{k l R}{2 \tau r^2} \sin 2x,$$

calling x the angle made by the sun with the first point of east. Likewise for the bifilar magnetometer we have

$$\Delta b = \frac{k l R}{\tau r^2} \sin x \cos y,$$

y being the angle between the sun and the fourth point of the horizon; that is, the angle between the sun's radius vector and the axis $o\,y$. Finally, for the vertical-force magnetometer, in the prime vertical

$$\Delta_0 = \frac{k l R}{r^2} \sin x \sin y \sin h',$$

the angle h' being given from the equation $\tan h' = \cot b \sin a$. These formulæ give for the declinometer a periodical variation,

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which is double during a day everywhere. But for the bifilar magnetometer they show that on two occasions its period, which is generally also a double, is reduced to a simple one, viz., when $L = o$ and $\delta = o$. The vertical-force magnetometer, also, has a simple period at the equator complementary to the bifilar, as appears clearly from the value of

$$P = \sqrt{X^2 + Z^2}$$

which for $L = o$ is reduced to

$$P = \frac{k R l}{r^3} \sin \delta,$$

which multiplied by sin h', gives a simple period. In any other place the period is always double. These conclusions agree with the Observations, and explain the march of the components of horizontal and vertical force at St. Helena; and they exhibit the simple period of the bifilar magnetometer at the equinoxes, which is sufficiently well proved from the observations. These formulæ give for the day and night equal excursions, because they make abstraction of the influence of the interposition of the terrestrial globe during the night-time. This being magnetical, diminishes strongly the solar influence. Hence, the night excursions exist, but exceedingly much diminished in extent. We can apply the same formulæ to this case also, if we suppose the distance of the needle much increased, so that the night-range should become $\frac{1}{6}$ or $\frac{1}{7}$ of the day-range, as is manifest from the figures given by Sabine, *Hob. Observ.* (vol. i. p. 48, plate.) This also may explain some irregularities in the day-period; since a period begun, when the earth is not interposed (the needle not being yet in the shadow), will be partly destroyed by the supervening discontinuity of action. This, practically, must be very often the case, since the co-ordinate axes, which have been supposed to be directed according to the fundamental points of the astronomical horizon, must be rather directed towards the cardinal points which refer to the magnetic axis of the earth, and then w must be considered and computed from the sun's passage at the magnetic meridian. Our formulæ may explain also the singular fact of irregular perturbations of the needle happening at certain fixed hours. This phenomenon, although supposed to be connected with meteorological causes, is evidently also in connexion with solar periods, so that the maxima of perturbation arrive at the hours of maxima and minima of nocturnal periods. This is the case with the nine o'clock perturbations. To explain all the phenomena of diurnal variation is certainly an attempt superior to the state of our actual knowledge of magnetism, since this explanation depends upon the law of distribution of magnetism on the earth and the sun, and upon the law of magnetical induction, both which are completely unknown to us; what I have exposed here is only to try if, by means of the known laws of nature, we can thrown any light on this difficult subject. It is obvious that I do not mean to exclude

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variations of magnetism due to meteorological influences and auroras, or to any other cause of irregular perturbation, since I have taken into consideration only the effects of the sun, and even neglected some small parts and coefficients, which may become sensible perhaps in individual cases, and explain minor periods.

"*Roman College Observatory,*
"*Rome, Sept.* 9, 1854."

On Saturn's Rings. By J. R. Hind, Esq.

"In the *Monthly Notice* of this Society for June 1853, is given an extract from a letter of Sig. Secchi's, addressed to Capt. Manners, R.N., referring to a drawing of *Saturn's* ring, executed by Campani at Rome in 1664, and considered to afford confirmatory evidence of that remarkable conclusion deduced by M. Otto Struve, — that the ring is contracting upon the ball at a rate which at no distant period may bring it into contact with the globe.

"I have not seen it noticed anywhere in print, that the first volume of Lubienietski's *Theatrum Cometicum* contains a well-engraved sketch of the appearance of *Saturn* and his ring, no doubt founded upon the drawing, the original of which was forwarded by Signor Secchi to Capt. Manners.* It accompanied a letter from Father Athanasius Kirch to Lubienietski, dated Rome, 1665, July 25, and is thus alluded to in that communication,— '*Mitto hisce inclusum Systema Saturninum à Campano ingeniosissimo Telescoplopœo* 50 *palmorum tubo observatum.*' It appears to have been taken at the end of July 1664, when the ring was very open. On measuring the relative breadths of the ring and the dark space between the interior edge and the ball, the latter is seen to be very slightly the broadest. Reducing the dimensions to seconds of arc with M. Otto Struve's value for the equatoreal diameter of the globe, I find for the breadth of the rings 5″·58 and of the dark space 5″·77. These numbers agree very well with the relative breadth inferred from a remark of Huyghens, in his *Systema Saturnium*, applicable to the year 1657, — that the breadth of the interval between the interior border of the ring and the body of the planet was equal to, or perhaps rather exceeded, the breadth of the ring. M. Otto Struve argues that this observation of Huyghens, when taken in comparison with recent measures, is alone sufficient to prove that a very considerable change has occurred in the system of the rings since the middle of the seventeenth century: Campani's drawing is certainly rather favourable than otherwise to this view.

"The roughly executed diagram of *Saturn* and his ring, at the time of the greatest inclination of its plane to our line of vision in July 1667, found in Lemonnier's *Histoire Céleste*,—

* See *Monthly Notice* for June 1853.

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though apparently less deserving of confidence than that given by Lubienietski, may be worth mention, as the evidence it affords, whether reliable or not, tends to support M. Otto Struve's inference. In this sketch the breadth of the dark space, compared with that of the rings, is about as 3 to 2. The diminished breadth of the space in the diagrams which follow may be accounted for by the closing up of the rings and the imperfections of telescopes in those times. It should be remarked, however, that Picard's avowed object in presenting this drawing, taken in July 1667, was to point out the extension of the ring beyond the edge of the globe, and had no reference to the comparative breadths of the ring and dark interval.

"I have found amongst Picard's observations on the ring, in the same work, a notice of that second dark belt, traversing the globe at the interior edge of the rings, which, with every appearance of probability, has been identified with the obscure ring of modern observers. M. Otto Struve alludes to observations of this nature made early in the eighteenth century; but probably Picard's had escaped his notice. Under date 1673, June 15, it is remarked, '*Saturne étoit sorti des raïons du soleil: il y avoit deux barres noires qui marquoient les deux bords intérieurs et extérieurs de l'anneau.*' A rough sketch is annexed to illustrate the observation. It appears difficult to account for this belt on any other assumption than the above; and we have, therefore, reason to suspect the existence of the obscure ring, at least as far back as 1673, notwithstanding the negative evidence which the subsequent observations of Sir W. Herschel and others may be considered to afford.

"At page 25 of the *Histoire Céleste* are some measures of the diameter of the ring by the same observer in the years 1666 and 1667, and approximate determinations of the angle of position of the greater axis with respect to the meridian. On the following page are also measures of the diameter of *Jupiter*, between October 3d, 1666, and November 10th, 1667; yet, strange to say, Picard does not appear to have remarked the compression at the poles during the interval over which these measures extend, since, in a note dated 1673, April 13, he says, 'I began to perceive that the disk of *Jupiter* was a little oval, and that the greater diameter is always in the direction of the belts.' It is uncertain, therefore, in what direction the planet was measured; but to obtain something like a probable value of the major axis of *Saturn's* ring in 1667, I have compared the measures of *Jupiter* with the mean results of Struve and others, assuming that Picard's numbers represent the *mean* diameters of the planet, and thus have deduced a correction to be applied to the measures of *Saturn*. In this way I find the exterior diameter of the ring $= 44''{\cdot}1$ (for *Saturn's* mean distance). If we compare with the values given by the observations of diameter with the meridian instruments at the Royal Observatory, this number would be increased to $47''{\cdot}1$. In either case it is larger than the measures of Bessel, O. Struve, and

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others, now indicate,—a mean of their results being 39″·5 at the same unit of distance. So far, then, as Picard's observations may be considered deserving of reliance, there is evidence that the exterior diameter of the ring is diminishing, with the gradual approach of its inner edge to the ball: the rate of contraction being apparently about 2″ in a hundred years. In saying thus much, however, I am well aware that great uncertainty attaches to any conclusion that we can arrive at on this subject, with the data at present before us."

Those who are interested in the curious theory respecting *Saturn's* rings, recently propounded by M. Otto Struve, and who are of opinion that the observations of the seventeenth century may be to some extent legitimately employed in testing that theory, will find an original representation of the planet and its appendage by Campani, in the MS. *Letter Book* of the Royal Society (vol. i. p. 324). This drawing, which appears on a small slip of paper carefully inserted in the volume, accompanies a letter from Auzout to Oldenburg, in which the writer states that it had been forwarded by Campani himself in a letter to Cassini, who was then residing at Paris. It is dated October 5, 1665. There are also two drawings of *Saturn*, as seen by Cassini in the years 1676 and 1677, to be found at page 584 of vol. x. of the *Hist. Anc. Acad. des Sciences.* It may be remarked that the interesting observation of Picard's respecting the spheroidal figure of *Jupiter*, which is cited by Mr. Hind in the foregoing paper, is the earliest *distinct* recognition of a real ellipticity of any of the celestial bodies which is to be found in the records of astronomical science, as the writer of this note has recently shown, it is to be hoped satisfactorily, in a paper *On the Early History of the Researches of Astronomers relative to the Spheroidal Figure of the Earth*, which is inserted in the *Monthly Notice* for June of this year (vol. xiv. p. 232), and to which the reader is referred for various other original notes and observations of a similar kind to that of the French astronomer. The remark of Picard is the more creditable to the acuteness and sagacity of that admirable astronomer, when it is considered that hitherto no other individual, with the exception of perhaps Cassini, appears to have entertained the remotest suspicion of the ellipticity of the planet, notwithstanding that it had been the object of continual scrutiny by observers since the invention of the telescope about the beginning of the seventeenth century. As regards Cassini, it may be stated, that, down to the year 1690, he had not entertained *a decided conviction* of the ellipticity of the planet, since in that year we find him still impressed with the idea that its disk was quite round, as is evident from the following passage extracted from the Registers of the Academy of Sciences of Paris for the year 1690:—" *M. Cassini remarqua à cette occasion que Jupiter, qui lui avait parût autrefois d'une figure un peu ovale, dont le*

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plus grand diametre tendait d'orient en occident lui paraissait à present parfaitement rond."—(*Hist. Anc. Acad. des Sciences*, vol. ii. p. 108.) In the very next year he announced definitively the spheroidal figure of the planet, and assigned to the ellipticity a value which appears to be about as good as any that has since been given.—EDITOR.

First Elements and Ephemeris of Polyhymnia,

Computed by Mr. George Rümker, from the following Observations :—

Paris, Oct. 29, 1854.
Berlin, Nov. 3,
Durham, Nov. 9,

Mean Anomaly	36° 2′ 51″·0	1854, Nov. 10·0, G.M.T.	
Perihelion	325 13 54·0	Mean Eq. 1854, Jan. 0·0	
Node	13 55 38·3		
Inclination	2 27 59·1		
Angle of Eccentricity	23 46 42·9		
Log. a	0·505894		
Log. μ	2·791166		

These elements represent the middle observation with errors,—
(Calculation − Observation) + 0″·2 in Longitude, 0·0 in Latitude.

The foregoing elements were accompanied with an Ephemeris of the planet, extending from Nov. 14 to Dec. 16.

Mr. E. B. Powell writes, that in his paper on the elements of Comet II. 1854 (see *Monthly Notices*, vol. xiv. p. 221), he unfortunately, by an oversight, inserted wrong numbers for the errors of anomaly. The following are the correct numbers and the resulting elements as now furnished by him :—

$\nu - u = -45''$ for the error of anomaly between the 8th and 13th
$\nu' - u' = -11''$ — — — 8th and 18th
$\nu'' - u'' = +(1' - 30'')$ — — — 8th and 27th

Taking the means of the results afforded by the angles for the two latter intervals, the following elements are obtained :—

Time of Perihelion Passage	= March 24·03468
Perihelion Distance	= ·2783
Inclination of Orbit	= 81° 44′
Longitude of Perihelion on Ecliptic...	= 170 16
Longitude of Ascending Node.....	= 316 0

Motion retrograde.

At the close of the meeting the President alluded to the curious theory which had been recently started, relative to the

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probability of a slow change continually going on in the position of *Saturn's* rings with respect to the body of the planet; and he pointed out the advantage of examining for this purpose the early observations of the rings, similarly, as on the present occasion, Mr. Hind had done in the paper which had just been read before the Society. He also expressed the interest which he felt in the paper communicated by Mr. Williams, illustrative of some points in Chinese Astronomy, remarking that while such researches were highly creditable to their author, they could not fail, if carefully prosecuted, to be attended with advantage to astronomical science. He then stated that there were three other circumstances to which he would allude in a few words. In the first place he would remark that the results of the Greenwich observations were now published in a separate form, and might be obtained by any person upon applying for them by letter addressed to the Royal Observatory. Secondly, he wished to direct the attention of observers to the fact that, on the Monday following (Nov. 13), the planet *Saturn* would approach very near a small star. He remarked that it was highly desirable to watch such close junctures, inasmuch as the observations of them might occasionally serve to throw light on the physical constitution of the rings of the planet. There was only one recorded instance of a star having been seen in the dark space between the rings and the body of the planet, but the evidence, in support of that observation, could not, perhaps, be regarded as sufficiently worthy of confidence. The President finally adverted, in very brief terms, to the series of pendulum experiments which had been recently executed under his superintendence at Harton Colliery, near South Shields, with the view of obtaining data for ascertaining the mean density of the earth, reserving, for a future occasion, a more detailed account of the operations. He alluded to the attempt of one of his predecessors, Dr. Maskelyne, to accomplish the same object by means of experiments on the deviation of the plumb-line from the vertical direction, occasioned by the attraction of the mountain of Schehallien, in Perthshire. He also noticed the experiments for the same purpose with the torsion balance, adding, however, that he did not consider them to be sufficiently trustworthy, in consequence of the numerous disturbing influences to which operations of so delicate a nature are liable. He then adverted to the experiments with the pendulum, which his friend, Dr. Whewell, and himself had made in the years 1826 and 1828, in the bottom of Dolcoath mine, Cornwall, with the view of obtaining a solution of the same important problem. Notwithstanding several obvious disadvantages under which they laboured, they persevered in the pursuit of their object, until their labours were definitively interrupted by the flooding of the mine. He remarked that in conducting experiments of this nature, there were three essential points to be attended to.—First, there was the comparison of the two pendulums (one of which was to be used at the bottom of the mine and the other at the top) in order to ascertain their physical difference: this was satisfactorily effected by an interchange of the

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positions of the two pendulums, any difference in their rate of oscillation being thereby completely eliminated. Secondly, there was the comparison of each pendulum with its clock ; this object was attained by employing the method of coincidences originally suggested by Captain Kater. Thirdly, an accurate comparison of the clock at the bottom of the mine with the clock at the top was indispensable. This was by far the most difficult part of the operations. In the Dolcoath experiment this object was effected by the very inconvenient process of carrying chronometers from the top of the mine to the bottom along a series of ladders. The application of galvanism to astronomical observations had suggested to the President the advantage of comparing the two clocks by effecting a galvanic connexion between them. This is accordingly the method which has been practised in the Harton Colliery experiments : it would be hardly necessary to add that it was, in every respect, superior to any other mode of comparison. The President finally remarked, that until all the necessary calculations were worked out, it was impossible to ascertain the definitive result of the experiments.

Note on the Origin of the Attempts made in the seventeenth Century to derive from physical Principles an invariable Standard of Measure. By the Editor.

The only physical standards of measure, which have been applied to astronomical and other scientific purposes in modern times, are those which have been furnished by the oscillations of the pendulum and the length of an arc of the meridian. This note is accordingly confined to some remarks on the origin of these two methods.

The pendulum seems to have *first* suggested itself to scientific men as capable of furnishing a permanent standard of measure. Sprat, in his History of the Royal Society (1667), enumerating the inventions and discoveries due to one or other of the Fellows of the Society, mentions the invention of "*a universal standard or measure of magnitudes by the help of a pendulum, never before attempted.*"—(4th Edition, p. 247). In a subsequent part of the same work the author gives a special account of the labours of Dr. (afterwards Sir Christopher) Wren, in the course of which he states that the latter had shown "*that there may be produced a natural standard for measure from the pendulum for vulgar use.*"—(p. 314.)

I am not acquainted with any author who has since alluded to the labours of Wren or any other Fellow of the Royal Society in connexion with this subject. The primary idea of employing the oscillations of the pendulum to obtain a standard of measure is not generally adverted to by scientific writers. Delambre, in his Analysis of the *Horologium Oscillatorium*, pub-

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lished in 1673 (*Hist. Art. Mod.* vol. ii.), notices the proposal of Huyghens to establish an invariable standard of measure on the basis of the oscillations of the pendulum; but he does not assert that the suggestion of such a measure was due to the Dutch philosopher. On the contrary, he refers to a somewhat similar proposal made by Mouton in his work — *Observationes Diametrorum Solis et Lunæ*, Lugd. 1670. It has, however, been recently asserted, although in somewhat obscure terms, upon the strength of the passage in the *Horologium Oscillatorium*, above alluded to, that the invention is really due to Huyghens. This point will be best ascertained by citing the words of the author himself. Referring to the advantage of a knowledge of the centre of oscillation, in finding the length of the pendulum, he thus remarks:—"Hinc necesse fuit illis, qui, ante hanc centri oscillatorii determinationem mensuræ universalis constituendæ rationem inierunt; *quod, jam inde à prima Horologii nostri inventione, nobilis illa Societas Regia Anglicana sibi negotium sumpsit* et recentius doctissimus Astronomus Lugdunensis, Gabriel Moutonus; his, inquam, necesse fuit designare globuli suspensi diametrum, vel proportione certa ad fili longitudinem cujus nempe tricessimam vel aliquam partem æquaret."— (*Horologium, Pars Quarta*, Prop. xxv.) It appears clearly from the foregoing passage that the idea of deriving an invariable standard of measure from the oscillations of the pendulum originated with the Royal Society, having been suggested to some of the Fellows by Huyghens' application of the pendulum to clocks.

Having recently had occasion to examine the MSS. of the Royal Society, I availed myself of the privilege so liberally accorded to me by that body, to ascertain if any records existed of the early labours of the Society in connexion with the establishmont of an invariable standard of measure. In this research I was not disappointed. The following extract of a letter from Huyghens to Sir Robert Moray, dated the Hague, Dec. 30, 1661, confirms the statement above cited from the *Horologium Oscillatorium:* "La fabrique de ma machine [pneumatique] m'a empeché quelque temps de travailler aux traités dont vous me demandez de nouvelles. J'ai entre les mains celui de l'horloge, duquel une grande partie est dediée aux mouvements, *et particulièrement j'y ai parlé de cet usage du pendule pour la mesure universelle, dont vous dites qu'on a traité dans votre assemblée.*" (*Letter Book*, vol. i. p. 22.)

Upon referring to the *Journal Book* of the Royal Society, I found that the question of an invariable standard of measure, founded on the oscillations of the pendulum, had been discussed at several of the meetings of the Society, towards the close of the year 1661 and the beginning of the following year. At the meeting, held on the 22d of January, 1662, it is stated that the President, Lord Brouncker, "introduced the history and schemes of the pendulum experiment, and that a committee, consisting of the President, Mr. Boyle, Sir William Pettey, Dr. Wilkins, and

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Dr. Wren, was appointed to make trials of it." (*Journal Book*, vol. i. p. 46.)

It would appear that the experiments of the Committee were not successful, for, at the meeting held on Feb. 5, 1662, "*Dr. Wren was entreated to think of an easy way for a universal measure, other than the pendulum.*" (*Journal Book*, vol. i. p. 48.)

Viewing the statement of Sprat, in connexion with this passage, it appears extremely probable that Wren was the first who proposed the deriving a standard of measure from the oscillations of the pendulum. The idea was, indeed, worthy of the genius of that distinguished man.

The difficulties experienced by the Committee, doubtless, arose from their ignorance of the centre of oscillation of the different pendulums, as well as from the disturbing effects of the resistance of the air and various other causes.

At length the discovery of a method for determining the centre of oscillation by Huyghens, removed what was considered to be the sole impediment to the success of the pendulum experiment in furnishing an invariable standard of measure. Huyghens announced his discovery in a letter to Sir Robert Moray, dated Nov. 21, 1664. He gives the rule for finding the centre of oscillation of a pendulum, consisting of a homogeneous spherical ball, suspended by a thread or wire. He states that for a pendulum oscillating half-seconds, he found by calculation that the distance between the point of suspension and the centre of oscillation was exactly $9\frac{1}{2}$ inches, Rhinland measure. (*Letter Book of the Royal Society*, vol. i. p. 225.)

The letter of Huyghens, above referred to, was read at the meeting of the Royal Society, on the 23d of November, 1664. In the *Journal Book* it is simply recorded that the letter was read; but there is evidence to show that the discovery, announced by Huyghens, was justly appreciated by the Society. Oldenburg, in a letter to Boyle, dated Dec. 3d, 1664, states that when the letter of Huyghens was read to the Society, "order was given that it should be punctually registered, to the end that Mr. Huyghens might have his due, and his inventions recorded for his honour to posterity." (Boyle's Works, vol. v. p. 328.)

The Society lost no time in verifying the theorem of Huyghens, respecting the centre of oscillation, by making experiments with pendulums of different lengths, and employing oscillating spheres of different magnitudes. In the first experiments with pendulums, oscillating in half-seconds, the theorem of Huyghens was found to "*be very near the truth.*" In the second series of experiments, when the oscillations of the pendulum were performed in seconds, it was found on one occasion that the string, with the smaller sphere attached to it, was $\frac{1}{10}$ *longer than it ought to have been, according to Huyghens' theorem.* A similar result was obtained in other instances wherein spheres of different magnitudes were employed. This inconsistency between theory and the results of actual experiment was attributed to the resistance of the air.

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Thus, notwithstanding the beautiful discovery of Huyghens, respecting the centre of oscillation, it was found that the oscillations of the pendulum could not be confidently employed in furnishing an invariable standard of measure. Hooke, who, probably, felt somewhat piqued, on account of the small share which he had in the inquiry, was the most strenuous objector to the use of this method. He remarked, among other disadvantages which would accompany its practical application, that if, as was in all probability the case, the force of gravity increased in intensity from the equator to the poles, it would necessarily follow that *in proceeding from the equator towards either of the poles, the oscillations of the seconds' pendulum would gradually be quickened.* This suggestion, so characteristic of the genius of Hooke, was announced to the Royal Society nine years previous to the return of Richer from Cayenne.*

Mouton (*Observationes Diametrorum*, &c., 1670) appears to have first proposed the length of an arc of the meridian as the basis of an invariable standard of measure. In the Appendix, p. 427, he suggests a decimal system of measures, the fundamental unit of which he proposes to derive from the value of a minute in Riccioli's length of a degree of the meridian; and he finds the unit of measure † thus obtained to be equal to the length of a simple pendulum, which makes 1252 oscillations in half-an-hour.

I am indebted to Mr. Sheepshanks for the foregoing statement respecting the proposal of Mouton to derive a standard of measure from the length of an arc of the meridian, not having myself seen the work in which reference is made to that method. ‡

It was soon suspected, as in the case of the pendulum, that the method of deriving a standard measure from the length of an arc of the meridian might be liable to serious objections. At the meeting of the Royal Society, held on the 7th of June, 1682, when the question of measuring an arc of the meridian in England was discussed, the President (Sir Christopher Wren) remarked that, according to his opinion, the best standard of measure "would be a certain part of the length of a degree upon the earth, *if at least upon several accurate trials of the measure of a degree in several latitudes it should be found the same, and not different, as it would be if the body of the earth were oval and not perfectly globular.*" (*Journal Book*, vol. vii. p. 86.)

It appears somewhat strange that it did not occur to any one that the supposed defects of the methods based upon the pendulum and the length of an arc of the meridian might be obviated by confining the experiments and observations to a fixed latitude.

* Hooke's objections to the use of the pendulum, in furnishing an invariable standard of measure, were stated at the meeting of the Society, held on Dec. 14, 1664.

† Termed by him *the Virga*; it was equal to the thousandth part of the length of a minute of Riccioli's arc.

‡ Delambre has justly remarked that Mouton's work is very rare.

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The planet (31), discovered at Washington, U.S., by Mr. Ferguson, on September 1, has been named by him *Euphrosyne.*

The *Nautical Almanac* for the year 1858 has recently been published. In addition to the usual matter there is a Supplement, containing approximate Ephemerides of the newly-discovered planets, down to *Amphitrite* (29), for the year 1855, and also an ephemeris of De Vico's Comet for the same year. The Supplement also includes numerical values of the elements of the four minor planets, — *Thetis* (17), *Lutetia* (21), *Calliope* (29) and *Amphitrite* (29).

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *December* 8, 1854. No. 2.

A. K. BARCLAY, Esq., V.P., in the Chair.

On the Orbit of 70 *Ophiuchi.* By Eyre B. Powell, Esq.

"Having been for some time engaged in investigating the orbit of the companion to 70 *Ophiuchi*, I have, at length, arrived at an ellipse which represents with considerable accuracy the angular motion of that interesting binary star; and I now do myself the pleasure of communicating my results to the Society.

"The process by which I obtained a first approximate orbit was the graphical one invented by Sir J. Herschel: three corrections then gave me the following elements:—

$$\tau = 1806{\cdot}82$$
$$\varpi = 291^\circ\,40'$$
$$\gamma = 46\ 2$$
$$\Omega = 294$$
$$e = {\cdot}566$$
$$n = -3^\circ{\cdot}676, \text{ and therefore } P = 97{\cdot}93 \text{ years.}$$

"The errors of position afforded by these elements for 1779, 1804, 1825, 1830, 1833, 1842, and 1852, are all moderate, scarcely ever exceeding a degree; but the discrepancy between the observed and computed positions for 1802 reaches the enormous amount of eight degrees. It is worthy of notice that, while the angle for 1802 is so erroneous, the position for 1804 differs by only 8′ from the result of observation.

"The subjoined table contains the variations of the angles of

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position, at eight different epochs, corresponding to changes in the preceding elements:—

Table of Variations of Position.

(Variations in minutes of space.)

Epoch.	τ	ϖ	γ	Ω	e	n
1779·77	$+\frac{33a}{4}$	$+\frac{71b}{60}$	$+21k$	$-\frac{67d}{5}$	$+\frac{61f}{2}$	$+\frac{25e}{6}$
1802·34	$+\frac{281a}{4}$	$+\frac{49b}{30}$	$-\frac{73k}{2}$	$-\frac{156d}{5}$	$+125f$	$+\frac{25e}{2}$
1804·42	$+\frac{257a}{4}$	$+\frac{72b}{60}$	$-\frac{7k}{2}$	$-\frac{41d}{5}$	$+69f$	$+\frac{25e}{6}$
1825·56	$+14a$	$+\frac{81·5b}{60}$	$-33k$	$-\frac{79d}{5}$	$-53f$	$-25e$
1830·5	$+9·5a$	$+\frac{71b}{60}$	$-27k$	$-7d$	$-37f$	$-\frac{25e}{2}$
1833·5	$+\frac{33a}{4}$	$+\frac{68b}{60}$	$-\frac{47k}{2}$	$-5d$	$-30f$	$-\frac{25e}{2}$
1842·55	$+\frac{23a}{4}$	$+\frac{62b}{60}$	$-\frac{27k}{2}$	$-\frac{3d}{5}$	$-15f$	$-\frac{25e}{3}$
1852·75	$+5a$	$+b$	$-3k$	$+\frac{2d}{5}$	$-\frac{13f}{4}$	$-\frac{25e}{2}$

a being tenths of a year; *b*, minutes of space; *k*, degrees; *d*, degrees; *f*, hundredths of unity; and *c*, minutes of space.

"The inspection of the variations thus drawn together affords hints with regard to the management of the equations of correction for the different elements. Such equations were formed and combined in a variety of ways, so as to make the values of the corrections depend chiefly on those epochal equations in which the corresponding coefficients were largest. The method of least squares is commonly used to attain the greatest accuracy; but it seemed to me that the foregoing plan would be sufficient to answer the purpose in hand. Thus, then, new and more correct values of the elements were found; but, as a comparison of the angles of position afforded by them, with those of observation, was not so satisfactory as I wished, I again altered some of the elements, and finally adopted the following:—

τ = 1806·92
ϖ = 291° 40′
γ = 49 56
Ω = 296 30 and λ = 7° 29′
e = ·546
n = −3°·668, and therefore P = 98·146 years.

"The length of the period is, I believe, considerably greater than any hitherto obtained; but an examination of the angular velocities of the star bears out my result.

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From 1833	to 1853	ϑ Position	= about	19
1842	— 1852	——	= —	$8\frac{1}{2}$
1846·2	— 1852·7	——	= —	6

"Now, to complete a whole revolution, so as to reach a position angle of 90°, about $23°\frac{1}{2}$ have to be described; and this will require at least 23 years. Hence, as 74 years have already elapsed, the periodic time cannot differ much from 98 years, and is, probably, somewhat greater.

"I annex a comparison of the observed angles of position with those given by the preceding elements.

Table of Comparisons of Observed and Computed Positions.

Date.	Position Observed.	Position Computed.	$P_c - P_o$.	Observer.
	° ′	° ′	° ′	
1779·77	90 0	89 4	— 56	$\mathrm{H}\frac{\mathrm{H}}{\mathrm{H}}$
1802·34	334 38	336 10	+1 32	$\mathrm{H}\frac{\mathrm{H}}{\mathrm{H}}$
1804·42	318 40	315 27	−3 13	$\mathrm{H}\frac{\mathrm{H}}{\mathrm{H}}$
1821·72	157 39	157 19	— 20	Σ
1822·6	153 54	154 25	+ 31	Σ
1825·56	148 12	146 36	−1 36	Σ & S
1828·67	140 18	140 25	+ 7	Σ & H
1830·5	137 28	137 25	— 3	B, H, & D
1832·561	134 21	134 26	+ 5	H & D
1835·56	130 36	130 37	+ 1	Sm
1838·51	126 30	127 19	+ 49	Sm
1842·55	122 24	123 17	+ 53	Sm
1846·017	120 31	120 7	— 24	J
1850·571	116 6	116 12	+ 6	J & F
1852·75	114 3	114 22	+ 19	J
1854 081	113 39	113 16	— 23	J

$\mathrm{H}\frac{\mathrm{H}}{\mathrm{H}}$, Sir W. Herschel; Σ, Professor Struve; S, Sir J. South; H, Sir J. Herschel; B, Professor Bessel; D, the Rev. Mr. Dawes; Sm, Admiral Smyth; J, Captain Jacob; F, Mr. Fletcher.

"The epochs pitched upon for the comparison were generally those for which there existed the most trustworthy observations; and in making the selection, I was guided, not only by the circumstances recorded of the measures, but by the angular velocities the latter afforded. Also, in combining the values assigned by different observers, I took the liberty of allotting weights according to the coherence and number of the sets of measures. The position for 1850 is a simple arithmetical mean between the angles of Captain Jacob and Mr. Fletcher, which differ *inter se* by nearly two degrees. I am inclined to think that Captain Jacob's angle for 1854 is rather too high; first, as the angular velocity obtained by combining it with his measure for 1852 is only three-tenths of a degree; and secondly, as I made the position for

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1853·586, by 59 fairly accordant observations, equal to 113° 35′, which is but 5′ less than the result of calculation.

"Using the elements and the measured distances for 1781, 1825, 1828, 1830, 1832, 1835, 1838, 1842, and 1850, I computed the corresponding values of a, the semi-axis major; the mean came out 4″·48.

"With this value of a the following table was formed, showing the comparison of the calculated and observed distances for thirteen epochs. The inclination of the true distance to the apparent one is included in the table, as it may be satisfactory to see its effect in fore-shortening the radius vector. The formulæ used were

$$\sin I = \sin \gamma \sin (v \pm \lambda), \quad \varrho = a(1 - e \cos u), \text{ and } r = \varrho \cos I.$$

Table of Comparison of Observed and Computed Distances.

Date.	ϱ, or Radius Vector.	I, or Inclination. ° ′	r_c, or Comp. Dist.	r_o, or Observ. Dist.	$r_c - r_o$.	Observer.
1781·74	5·687	31 9	4·867	4·49	+·377	H
1804·42	2·177	21 6	2·031	2·56	−·529	H
1825·56	4·873	30 48	4·186	4·00	+·186	Σ
1828·67	5·288	25 44	4·764	4·79	−·026	Σ
1830·5	5·509	23 0	5·071	5·478	−·407	B, Sm, & D
1832·561	5·736	20 6	5·386	5·456	−·070	H & D
1835·56	6·028	16 10	5·79	5·97	−·180	Sm
1838·51	6·273	12 35	6·123	6·351	−·228	B, Sm, & D
1842·55	6·543	7 59	6·48	6·682	−·202	Sm & D
1846·017	6·716	4 18	6·697	6·83	−·133	J
1850·571	6·864	0 30	6·864	6·86	+·004	J
1852·75	6·904	2 31	6·897	6·73	+·167	J
1854·081	6·918	3 50	6·903	6·365	+·538	J

"On glancing at the numbers in the sixth column, the arrangement of the signs of $r_c - r_o$ suggests the existence of error in the elements. It has been asserted that the distance is now decreasing; if this be correct, the node, or the peri-astre, or both, should be somewhat advanced. As, however, the measures of distance are undoubtedly less worthy of confidence than those of position, it appears unnecessary at present to modify the elements with the sole view of reconciling the distances. No weight can be attached to the distance of 1804, as it was obtained by mere estimation in diameters. In 1825 Professor Struve found the projected radius vector 4″·0, Sir J. South 4″·76; the true value in all probability lies between these determinations, and, perhaps, nearer the former than the latter. This opinion is grounded upon two facts; first, that on Professor Struve's testing his distances, he found them on an average about two-tenths of a second too small; and secondly, that the distance in 1828 was only 4″·79, while

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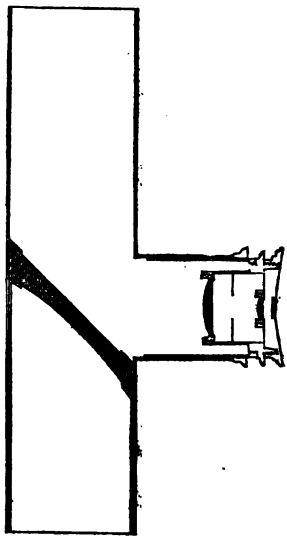
from the nature of the orbit the value of r was increasing pretty rapidly. For 1850 Captain Jacob and Mr. Fletcher differ by 0".366, and the distance by computation agrees nearly with that obtained by the former. The most offensive discrepancy in the table is that for the current year; but even here it seems probable the orbit is not so far in fault as might at first be fancied. The angular velocity is apparently as small, or even smaller, than it was prior to 1850, and the radius vector lies near both the axis major and the line of nodes; hence there can be but a slight change in the distance.

"Upon the whole, the preceding elements appear to represent the motion of 70 *Ophiuchi* tolerably well; and it is to be feared that, till the companion approaches its peri-astre, but little can be done to fix definitively the most important points connected with the orbit. Probably, however, three or four years' farther observations will put it in our power to improve the position of the ellipse with reference to the line of nodes.

"*Madras, August 9th*, 1854."

Description of an Eye-piece for Observing the Sun. By R. Hodgson, Esq.



"The novelty in the arrangement of this eye-piece is, that it permits the full aperture of the object-glass to be used while observing the whole of the sun's disk; an object of importance in solar eclipses and transits of planets across the sun; and it has also the further advantage of allowing the application of all the usual eye-pieces and micrometers.

"It consists in applying to the draw-tube of the telescope a diagonal glass reflector, about two inches in advance of the focal point; the first or reflecting surface being perfectly flat, and the second ground to a concave figure, and left unpolished: a dark glass is, of course, still necessary to protect the eye from the glare of the sun; but a light neutral tint is sufficient.

"I have applied it to my 6-inch equatoreal, and also tried it with equal success upon a telescope of 8 inches' aperture.

"*November* 1854."

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Note respecting the recent Pendulum Experiments in the Harton Colliery. By G. B. Airy, Esq., Astronomer Royal.

Since the last meeting of the Society, the computations of the pendulum experiments in the Harton colliery have been completed, and the result appears very satisfactory. The pendulums do not appear to have received the slightest injury or change during the operations of interchange of position. The final result for the pendulums is,—

Acceleration at the bottom of the mine is 2·25 vibrations per day ; or,

Increase of gravity at the bottom of the mine is $\frac{1}{19190}$ part of the whole.

Surveys will now be made for obtaining those data which are necessary in order to deduce, from this first result, the final result as to the earth's density. Until these are finished, no approximate result can be given with confidence : it appears likely, however, that the resulting density of the earth will be greater than those hitherto obtained.

December 8, 1854.

On the Satellite of Neptune. By J. R. Hind, Esq.

A discussion of the valuable series of observations of the satellite of *Neptune*, taken at Valetta in 1852, by Mr. Lassell, has led me to a result which may not be without interest, as hitherto only one instance of the kind was known to occur in the planetary system. There can be no doubt that the motion of the satellite is *retrograde;* and it offers a far more decided case of retrograde movement than the Uranian system, the inclination of the orbit of *Neptune's* attendant being only 29°, whereas the satellites of *Uranus* revolve in a plane which is inclined to the ecliptic more than 79°.

From a considerable number of observations I found the place of the ascending node, in the retrograde orbit, in longitude 175° 40′. It follows, therefore, from the present position of the planet in the ecliptic, that the apparent orbit of the satellite is gradually contracting, and will become a right line in 1859, when the line of nodes is directed towards the earth. Mr. Lassell has informed me that this is confirmed by his recent observations, in which the satellite was found to pass nearer to the planet than at the time of its discovery—a circumstance irreconcilable with *direct* movement, since, in that case, the place of the node would be behind the present position of *Neptune* on the ecliptic, and the apparent orbit of the satellite would necessarily be opening.

Another inference may be drawn from the observations of

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1852, namely, the existence of a very considerable ellipticity in the orbit, though, unfortunately, the determination of its precise amount, as well as a fair approximation to the position of the line of apsides, are scarcely practicable from present data; and since the apparent ellipse is rapidly approaching to a right line, we must wait 12 or 14 years before the measures necessary for completing the investigation can be obtained.

It will therefore be understood, that the position of the peri-neptunium and the angle of eccentricity in the following elements are very uncertain, though the inclination and node, and also the period of revolution, are, I believe, pretty exact.

Epoch 1852, November 0·0, Greenwich M.T.

Mean Anomaly	243° 32
Peri-neptunium	177 30
Ascending Node	175 40
Inclination	151 0
Angle of Eccentricity	6 5
Period	5^{d}·8769

If I am right in placing the lines of nodes and apsides nearly in coincidence, it will follow that, at the present date, there will be no very sensible difference between the extreme distances of the satellite eastward and westward of the primary, and this deduction appears to be borne out by a comparison of the measured distances at those points. It will also necessarily result that the satellite must pass nearer to the planet, when it is in the north-preceding quadrant, than it does in the opposite direction—a condition agreeing well with observation. At this epoch the satellite is nearest to the earth when it is to the north of its primary.

I subjoin an extract from a communication with which I was favoured by Mr. Lassell, affording strong evidence in support of the conclusions deduced above:—

"I certainly incline to the opinion that the apparent orbit of *Neptune's* satellite has diminished its conjugate axis since the earlier observations, though all but those of 1852 are made so near the greatest elongation that they could scarcely give even a rude approximation to the true form of the apparent orbit. * * I don't know whether it has occurred to you that the observations at Malta seem to indicate pretty clearly an apparent eccentricity of the orbit,—the satellite passes the planet at its nearest approach on the north-preceding side, something like a second *nearer* than at its nearest approach on the south-following side."

The semi-major axis of the satellite's orbit, as seen at the mean distance of *Neptune*, subtends an angle of 16″·98, according to a preliminary discussion of Mr. Lassell's Valetta measures, whence, with the periodic time I have given above, the mass of the planet is found to be $\frac{1}{17135}$, and the mean distance of the

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satellite from the centre of *Neptune*, 235,800 miles. I believe it will appear, from a comparison of this gentleman's measures generally with those of other observers, that they have a tendency to be in excess rather than in defect of the mean, and consequently the value of the mass, deduced from them, will possibly admit of some diminution: probably $\frac{1}{17500}$ will prove very near the truth.

December 7, 1854.

Note on the Satellites and Mass of Uranus. By J. R. Hind, Esq.

In the *Monthly Notice* of this Society for March 11th, 1853, appears an extensive set of measures of the two brighter satellites of *Uranus*, *Titania* and *Oberon*, taken by Mr. Lassell during his residence at Malta, under circumstances far superior to those which affect the accuracy of such observations in the climate of this country. I have lately worked up those measures with the view of determining the positions of the orbits of the satellites and the mass of their primary. My results are as follow:—

I. *Oberon.*

Radius of Orbit at the Mean Distance of Uranus ..... 45″·20
[or 385,000 miles]
Ascending Node ..... 165° 28′
Inclination 100 34

II. *Titania.*

Radius of Orbit at the Mean Distance of Uranus ..... 33″·88
[or 288,600 miles]
Ascending Node ..... 165° 25′
Inclination 100 34

It will be remarked that these observations give the position of the node and inclination almost precisely the same as they were found by Sir William Herschel more than half a century ago—his numbers being $\Omega = 165° 30'$, $i = 101° 2'$.

I find from the distances of *Oberon*, mass of *Uranus* $= \frac{1}{20642}$, and from those of *Titania* $\frac{1}{20505}$, adopting Mr. Adams' periods of revolution: the mean $\frac{1}{20570}$ agrees very nearly with the value derived from the measures of Sir W. Herschel.

Mr. Lassell's observations indicate with tolerable certainty that the mass of *Neptune* is greater than that of *Uranus* in the proportion of 7 to 6.

1854, *Dec.* 7.

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United States Expedition to Chili.

(*Extract of a Letter from Lieut. G. M. Gilliss, U.S.N. to Admiral W. H. Smyth.*)

"Preparatory to the publication of our volume of *Differential Observations on Mars and Venus in Chili*, we are discussing our longitude, and need for this purpose all the corresponding moon-culminations in Europe.

"Congress acted very liberally in the order to publish our results, directing that the whole work should be well printed on good paper, of quarto size, and neatly bound. In all there will be five volumes. Vol. i., containing a full account of Chili (except its political history), and Lieut. MacRae's narrative of the magnetical expedition across the Andes and Pampas of Buenos Ayres, will fill more than 700 pages. It has 51 maps and plates of landscapes, natural history, &c. These are in the engraver's hands, and the volume will probably be ready in July.

"Vol. ii., the differential observations on *Mars* and *Venus* wherever made and furnished us. We shall probably fill 500 pages, and the volume will be put to press by May.

"Vols. iii. and iv., meridian observations and zones, each 500 to 600 pages. The instrumental and clock errors have been computed for a large portion of the time; but it is not probable the volumes will be printed in less than $2\frac{1}{2}$ years. The reduction of more than 20,000 *new* stars and their arrangement in the order of right ascension from the zones is a work of much time.

Vol. v., magnetical and meteorological observations, will fill also 500 or 600 pages. This will appear shortly after vol. ii.

"You will perceive that we three in Chili accomplished no small amount of labour; I hope the results may prove acceptable to men of science.

"*Washington, 8th Nov.*, 1854."

On the Resistance of the Ethereal Medium and the Attraction of the Small Planets. By A. J. Angström, Esq., Junior Professor of Astronomy in the University of Uppsala.*

The planetary secular perturbations have more especially engaged the attention of astronomers, from the circumstance that the solution of questions, respecting the stability of our solar system,

* This paper is a translation from the Swedish, by A. D. Wackerbarth, Esq. who also communicated it to the Society.

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finally depends on the amount of these variations, which, however inconsiderable they may appear, may, in the lapse of ages, accumulate to such a degree, as entirely to change the character of the elements.

Both theory and experience, however, agree that perturbations of such a nature cannot take place within our planetary system. The exception to this rule, which, in the beginning of the present century, was supposed to have been found in the moon's motion, whereof the mean motion had, from the earliest period to which astronomical observations extend, continually increased, disappeared when Laplace succeeded in explaining that phenomenon by a slow change in the eccentricity of the earth's orbit. Hence it appeared that this variation was, like its cause, periodical, and did not even affect directly the mean velocity, but what is called the epoch.

The case is, however, otherwise with comets. Here the largeness of the inclinations and eccentricities by no means excluded perturbations, even of a periodic nature, of such extent as entirely to change the elements of the orbit. A striking example of this is afforded by Lexell's comet, and, probably, also the two comets named after Brorsen and D'Arrest. And such must generally be the case with any comet that comes into the neighbourhood of *Jupiter*. But if the comets can be disturbed in their orbits by the planetary attractions, on the other hand, the masses of the comets are so trifling, that they cannot exercise any sensible influence on the motions of the planets.

Nevertheless, one of the comets, namely that of Encke, has given rise to a supposition which, should it be confirmed, must eventually make itself evident in the motions of the planets, and form a real exception to the received law of the unchangeableness of the mean distances, as subjected only to periodic variations.

It has, in fact, appeared that this comet — independently of the considerable changes which the period undergoes from the planetary perturbations—has, at each revolution, increased its mean velocity in its orbit, so as each time to arrive at perihelion $2\frac{1}{2}$ hours earlier than on the preceding occasion.

This circumstance is easiest explained in the following manner, according to the evidence produced by the learned calculator of that comet, Professor Encke:—there enters, or may be supposed to enter, into the expression for the mean longitude, a term varying as the square of the time; but then comes the difficulty of accounting for the presence of that term by the general law of gravitation. This has not hitherto been done; and Encke has accordingly had recourse to another means of explanation, namely, the resistance of an ethereal medium.

It is known that the effect which a resisting medium, of the same nature as the atmosphere, would have on a heavenly body's motion, would be to reduce the periodic time and eccentricity of the orbit, whereby there must arise an augmentation in the mean velocity, continually increasing with the time. This was just

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what was wanted; and, on the strength of this experience, Encke assumed, in speciality for his ether, that its density decreases as the square of the distance from the sun; and, furthermore, that the resistance is proportional to the density of the ether and the square of the comet's velocity.

This supposition is, however, subject to weighty objections. First and principally, if the density of the medium increases up to the surface of the sun, it must hence follow that the ether itself participates in the solar system's progressive motion, as well as in the sun's rotation; for we have no right to ascribe to the ether the properties of air in one instance, and deny the consequences thereof in another. The sun's rotatory motion is then immediately communicated to the circumjacent condensed ether, and is thence communicated from stratum to stratum, with either the same or a diminished angular velocity. If we assume that the angular velocity diminishes in the same ratio as that of the planets, it follows that the resistance to the planets and to the comets of direct motion is nearly evanescent, but is so much the more increased in the case of comets with retrograde motion. But that this is not the case is sufficiently evidenced by Halley's comet.

The apparent diminution in the volume of the nucleus of Halley's comet, when in the neighbourhood of the sun, has been taken as evidence of the ether's increasing density. But as ether possesses the property of passing freely through all bodies, an increase in its density would not compress, but, on the contrary, if it had any effect at all, would expand bodies. The Newtonian theory for the origin of comets' tails, namely, that they are turned from the sun for the same reason as vapours rise in our own atmosphere, Bessel* has shown to be insufficient to account for the various changes in their form.

Since, then, on the one hand, on the hypothesis of the ether's condensation, its resistance, in the case of Encke's comet, must vanish, and, on the other, that hypothesis is insufficient to explain the phenomena as desired, it would seem that it ought to be rejected.

On the density of ether within diaphanous bodies, opinions, as we know, differ; since some have considered that density as constant, while others again, like Fresnel, conceive it to be different in different media; but even if the second of these opinions be the more probable,† and thus the molecular forces have the power of altering the ether's density, yet assuredly no such power can be ascribed to gravity.

No traces of the resistance of ether are discoverable in the motions of the planets, and this has been considered as arising from their greater density and less volume; but this is subject to much

* *Astron. Nach.* tome xiii. p. 185.

† See the Author's article on the "Plane of Polarisation." *Vetanskeps-Akademicus Förhandlingar*, 1853, No. 6.

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doubt. Fresnel's theory of aberration, as well as direct experiments by Fizeau,* seem to show that ether, to a certain degree, participates in a diaphanous body's motion, and that in proportion to the said body's refracting power. Thus, the ether's resistance takes place within the body itself, and must, therefore, be independent of the body's volume, but must increase in the ratio of its refracting power. But as this, on the other hand, is compensated by the greater mass which bodies of strong refracting power generally possess, it would seem that we are by no means at liberty to assume any great difference between planets and comets with respect to the ether's resistance.

Meanwhile our knowledge of the component parts of comets is extremely scanty. That they are not either solid or gaseous bodies, is evident from their property of not refracting light. They are, probably, of a powdery consistency. This we may conclude from their property of reflecting the sun-light, combined with the experience of certain observers,† that stars, seen through the nucleus itself, appear with even increased brightness. This last-mentioned phenomenon, may perhaps rest on an illusion, as others, for instance Bessel, have observed the contrary; but it may nevertheless be explained by the laws of the interference of light. For if a bright object be occulted by a body of small surface, that object may yet show itself with undiminished lustre behind the occulting body in the direction of the line of junction. Then if the occulting body be itself luminous, its light is added to that of the bright object, which is thus made to appear brighter.

As regards the assumption that the resistance varies as the square of the body's velocity, it is subject to the objection, that the notion of great or small can here only be applied to a velocity with reference to the medium in which the motion takes place; and thus that a velocity, which for air is considerable, cannot be so considered for ether. The speed with which these two separate media transmit undulations, is the best starting-point for a comparison. Thus it seems that the perihelion velocity of Encke's comet is barely one 5000th part of that of light, or about $\frac{1}{5}$ foot per second. This circumstance appears to me of especial weight, when one endeavours to reckon the resistance of ether, which again is still further modified by its probably arising within the body itself.

It would seem to follow, from the preceding considerations, that the density of the ether is throughout space the same, and that its resistance, if it really have any sensible influence, must vary as the first power of the velocity. Professor Hansen, who subjected the different hypotheses on the ether's resistance to a searching examination, has shown that the latter assumption makes but little difference in the numerical value of the resistance, but not so the

* *Comptes Rendus*, xxxiii. p. 349.
† Piazzi, Struve, Realhuber.

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former. For if we give the function (called by Encke U*) a value answering to the observed shortening of the periodic time, the perihelion distance decreases faster than it would if the ether's density increased in the neighbourhood of the sun, inasmuch as the eccentricity in that case diminishes more slowly. Encke has, indeed, with respect to the appearances of 1822 and 1832, when the comet was seen only after perihelion, uttered a suspicion, that the true law of the resistance is as yet undiscovered, and the deviations at the last appearance seem to confirm this: but whether the assumption of a constant density would represent the observations any better, cannot be decided beforehand. Still, to some extent, this would seem to be the case, since the observations of 1822 and 1832 tend to give the comet's orbit a greater eccentricity than what results when only observations before perihelion are used. They would, therefore, better harmonise with that hypothesis, as to the nature of ether, which makes the eccentricity decrease more slowly.

The objections to the hypothesis of the ether's resistance, which have been set forth in the preceding pages, have, however, awakened in my mind a lively wish to explain, by some other process, the acceleration which observations prove in the periodic time of Encke's comet; so much the more, as the celebrated proposer of that hypothesis himself remarks,—"dass das widerstehende Mittel nur als eine Form fur die nothwendig gewordene Correction anzusehen ist."†

The small planets, whose attractions have not hitherto been taken into account, have, in this respect, attracted my especial attention, and I here take the liberty of setting forth the results of the preliminary investigation that I have instituted on that subject. Even if it should appear that the observed inequality in the motion of Encke's comet cannot by this means be explained, yet every contribution to the knowledge of the mutual relations of these celestial bodies must possess at least some interest. Encke's comet will, assuredly, at some future time, throw light on the extent and masses of the small planets, and thus still further increase the number of truths wherewith it has already enriched astronomical investigations.

The number known of these bodies, from 4, which was the amount in the early part of the present century, has, within the last ten years, risen to 27. The rapidity with which these discoveries have followed one another, renders it probable that their real number is very great, although, on account of their smallness, the greater part of them are likely to remain unknown to us. Whether one adopt the Olbersian hypothesis, that all these asteroids are fragments of one large planet, or—be their origin what it may—that they form a sort of belt or ring

* [U is used by Encke for the portion of the disturbing force that arises from the resisting medium.— A. D. W.]

† *Astr. Nachr.*, xii. p. 321.

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between *Mars* and *Jupiter*, it is evident that their secular perturbations, which are independent of their places in their orbits, must be the same, whether we consider them as all united in one, or disposed upon several points of a common orbit. The secular perturbations, then, of Encke's comet by the small planets, may give the approximate value of their aggregate mass.

To ascertain the mean position of the 27 asteroids' orbits, I have, from their elements, calculated the mean value of the following quantities :—

$$a\,e\cos.\varpi, \qquad e\sin\varpi, \qquad \sin\Omega\tan I, \qquad \cos\Omega\tan I,$$

and hence obtained,—

For the Asteroids.		For Encke's Comet.	
$\varpi = 65^\circ\ 1'$	$e = 0{\cdot}0555$	$\varpi_1 = 157^\circ\ 18'{\cdot}4$	$e_1 = 0{\cdot}8478$
$\Omega = 138\ 23$	$a = 2{\cdot}567$	$\Omega_1 = 334\ 29{\cdot}5$	$a_1 = 2{\cdot}222$
$I = 4\ 19$		$I_1 = 13\ 20{\cdot}7$	

where the elements of Encke's comet are, for comparison's sake, added. Moreover, if we call—

The inclination of the comet's orbit to that of the mean asteroid ..... ι
The distance of the perihelion from the ascending node ..... ω
The same angle for the mean asteroid's orbit ..... ω_1

we have

$$\iota = 17^\circ\ 32' \qquad \omega = 186^\circ\ 47'{\cdot}5 \qquad \omega_1 = 94^\circ\ 21'$$

We see, then, that the two orbits form, as it were, two links in a common chain, and that their perihelia are nearly at right angles to one another. This, combined with the large value of ι, shows that the two orbits cannot come into very close quarters with each other. On examining the asteroids' orbits separately, we find that, for some of them whose node-lines fall in the first, and perihelion in the second quadrant, an approach to the comet's orbit is possible. I have, for several of them, reckoned the angles ω and ω_1, and hence the comet's and asteroids' radii-vectores in the line of nodes; but for only three has the difference been less than unity.

	ι	r	r'	Diff.
Irene	$18^\circ\ 48'$	2·361	2·811	0·450
Proserpine ...	15 52	3·192	2·662	0·527
Ceres.....	18 48	2·023	2·959	0·936

where r is the comet's, and r' the asteroid's radius-vector, in the line of the comet's ascending node. Hence we see that among the hitherto discovered asteroids, there is none that approaches sufficiently near to the comet's orbit to exercise any sensible influence on the comet's motion. It does not, however, hence

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follow that there may not be such among the yet undiscovered. Were there one with a mean distance of nearly 2·222, it would produce a periodic inequality of a secular nature, which, in spite of the asteroids' smallness, might be sensible on the integration of the differential formulas.

To return to the secular perturbations which the asteroids cause to Encke's comet. This belongs to the more difficult class of astronomical problems, inasmuch as the projections of the two orbits on the ecliptic intersect each other. Bessel* has indeed shown how to find the secular perturbations which a planet causes to a comet, or planet with a comet-like orbit, when the disturbing force can be developed in a converging series in powers of $\frac{r'}{r}$, if r and r' signify the radii-vectores of the comet and planet respectively; and Hansen† has discussed the opposite case in which the comet's radius-vector is always less than the planet's; but these methods both fail when the orbits cross one another.

The following method will not, then, be devoid of interest, although it must be owned that, when the mean distances of the two bodies are nearly equal, it does not lead to very rapidly converging series.

If we designate by m and m' the comet's and disturbing planet's respective masses, and their co-ordinates by

$$x, y, z, r, x', y', z', r',$$

and their distance from each other by ϱ, we obtain, as is well known, the components of the disturbing forces by differentiating the Lagrangian function,

$$R = -m' \cdot \frac{(x \cdot x' + y \cdot y' + z \cdot z')}{r'^3} + \frac{m'}{\varrho};$$

but as the secular perturbations are independent of the planet's co-ordinates, we can reject the first term of this expression, whence we have

$$R = \frac{m'}{\varrho}.$$

Since, moreover, in this expression those quantities only are to be retained which are independent of the planet's mean anomaly $n' t$, we may put

$$R_1 = \frac{m'}{2\pi} \int_0^{2\pi} \frac{dn't}{\varrho} \ddagger$$

* *Astron. Nachr.*, xiv., p. 1.

† *Ermittelung der absoluten Störungen in Ellipsen*, 1843.

‡ [If we developе $\frac{m'}{\varrho}$ in a series of multiples of the cosines of the mean heliocentric elongation, and then cast away all the terms involving $n't$, there will evidently remain only the series of constants expressing this definite integral.—A. D. W.]

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For the finding of this integral it is necessary to expand $\frac{1}{\varrho}$ in a converging series. For this purpose, if we call the angle between the radii-vectores U, we have

$$\varrho = \sqrt{(r^2 + r'^2 - 2 \cdot r \cdot r' \cdot \cos U)},$$

and can put

$$\frac{1}{\varrho} = \frac{1}{(r^2 + r'^2)^{\frac{1}{2}}} + \tfrac{1}{2} \cdot \frac{(2 \cdot r \cdot r' \cdot \cos U)}{(r'^2 + r^2)^{\frac{3}{2}}} + \frac{1 \cdot 3}{2 \cdot 4} \cdot \frac{(2 \cdot r \cdot r' \cdot \cos U)^2}{(r'^2 + r^2)^{\frac{5}{2}}} + \ldots \quad (a)$$

or generally,

$$\frac{1}{\varrho} = \frac{1}{(r'^2 + r^2)^{\frac{1}{2}}} + 2 \frac{1 \cdot 3 \cdot 5 \ldots (2 \cdot n - 1)}{2 \cdot 4 \cdot 6 \ldots (2 \cdot n)} \cdot \frac{(2 \cdot r \cdot r' \cdot \cos U)^n}{(r'^2 + r^2)^{n + \frac{1}{2}}}$$

where n takes all positive values from 1 to ∞.

The convergency of this series is determined by the expression $\frac{2 \cdot n - 1}{2 \cdot n} \cdot \frac{2 \cdot r \cdot r' \cdot \cos U}{r'^2 + r^2}$, which must always be less than unity, if r is either greater or less than r', and also even if $r = r'$, unless at the same time $\cos U = 1$. The series (a) may, therefore, be considered as always convergent, except in the case of a collision between the two celestial bodies, in which case, however, the differential equations also would cease to hold.

Since, for perturbations of the first order, if u' be the excentric anomaly

$$d \cdot n' t = \frac{r'}{a'} \cdot d u'$$

we have $$\frac{1}{2 \cdot \pi} \cdot \int_0^{2\pi} \frac{d \cdot n' t}{\varrho} = \frac{1}{2 \cdot \pi} \cdot \int_0^{2\pi} \frac{r'}{a' \cdot \varrho} \cdot d u' = \frac{R_1}{m'}.$$

It is not necessary for the determination of R_1 to avoid developments proceeding according to the ascending powers of the planet's excentricity e'. If, then, we put

$$\frac{1}{m'}, R_1 = \frac{V^0}{(a'^2 + r^2)^{\frac{1}{2}}} + \frac{V^I}{(a'^2 + r^2)^{\frac{3}{2}}} + \frac{V^{II}}{(a'^2 + r^2)^{\frac{5}{2}}} + \frac{V^{III}}{(a'^2 + r^2)^{\frac{7}{2}}} + \text{\&c.} \quad \ldots \quad (b)$$

and reject terms multiplied by e'^3 and the higher powers, we have

$$V^0 = 1$$

$$V^I = -\frac{2}{3} \cdot a' \cdot e' \cdot p - \frac{3}{4} \cdot a'^2 \cdot e'^2$$

$$V^{II} = \frac{3}{4} \cdot a'^2 \cdot r^2 \cdot \{(p^2 + q^2) + e'^2 \cdot (4 \cdot p^2 - q^2)\} + \frac{3}{2} \cdot a'^3 \cdot r \cdot e' \cdot p + \frac{3}{4} \cdot a'^4 \cdot e'^2$$

$$V^{III} = \frac{-75}{16} \cdot a'^3 \cdot r^2 \cdot e' \cdot p \cdot (p^2 + q^2) - \frac{15}{32} \cdot a'^4 \cdot r^2 \cdot e'^2 \cdot (25 \cdot p^2 + 2 \cdot q^2)$$

$$V^{IIII} = \frac{105}{64} \cdot a'^4 \cdot r^4 \cdot (p^2 + q^2)^2 + \frac{1}{4} \cdot a'^4 \cdot r^4 \cdot e'^2 \cdot (18 \cdot p^4 + 15 \cdot p^2 \cdot q^2 - 3 \cdot q^4)$$
$$+ \frac{105}{64} \cdot a'^5 \cdot r' \cdot e' \cdot p^3 \cdot (p^2 + q^2)$$

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and so on, or if we preserve only the first power of e',

$$\frac{1}{m'}\,R_1 = \left.\begin{aligned} &\frac{1}{(a'^2+r^2)^{\frac{1}{2}}} + \frac{1\cdot3}{2\cdot4}\cdot\frac{1}{2}\cdot\frac{a'^2\cdot(p^2+q^2)\cdot2^2\cdot r^2}{(a'^2+r^2)^{\frac{5}{2}}} + \frac{1\cdot3\cdot5\cdot7}{2\cdot4\cdot6\cdot8}\cdot\frac{1\cdot3}{2\cdot4}\cdot\frac{a'^4\cdot(p^2+q^2)^2\cdot2^4\cdot r^4}{(a'^2+r^2)^{\frac{9}{2}}} \\ &+\frac{1\cdot3\cdot5\cdot7\cdot9\cdot11}{2\cdot4\cdot6\cdot8\cdot10\cdot12}\cdot\frac{1\cdot3\cdot5}{2\cdot4\cdot6}\cdot\frac{a'^6\cdot(p^2+q^2)^3\cdot2^6\cdot r^6}{(a'^2+r^2)^{\frac{13}{2}}} + \&c. \quad . \quad . \quad . \\ &-\frac{1}{2}\,\frac{3}{2}\cdot\frac{a'\cdot e'\cdot p\cdot2\cdot r}{(a'^2+r^2)^{\frac{3}{2}}} - \frac{1\cdot3\cdot5}{2\cdot4\cdot6}\cdot\frac{3\cdot5}{2\cdot4}\cdot\frac{a'^3\cdot e'\cdot p\cdot(p^2+q^2)\cdot2^3\cdot r^3}{(a'^2+r^2)^{\frac{7}{2}}} \\ &-\frac{1\cdot3\cdot5\cdot7\cdot9}{2\cdot4\cdot6\cdot8\cdot10}\cdot\frac{3\cdot5\cdot7}{2\cdot4\cdot6}\cdot\frac{a'^5\cdot e'\cdot p\cdot(p^2+q^2)^2\cdot2^5\cdot r^5}{(a'^2+r^2)^{\frac{11}{2}}} - \&c. \quad . \quad . \quad . \\ &+\frac{3}{2}\cdot\frac{1}{2}\cdot\frac{a'^3\cdot e'\cdot p\cdot2\cdot r}{(a'^2+r^2)^{\frac{5}{2}}} + \frac{3\cdot5\cdot7}{2\cdot4\cdot6}\cdot\frac{1\cdot3}{2\cdot4}\cdot\frac{a'^5\cdot e'\cdot p\cdot(p^2+q^2)\cdot2^3\cdot r^3}{(a'^2+r^2)^{\frac{9}{2}}} \\ &+\frac{3\cdot5\cdot7\cdot9\cdot11}{2\cdot4\cdot6\cdot8\cdot10}\cdot\frac{1\cdot3\cdot5}{2\cdot4\cdot6}\cdot\frac{a'^5\cdot e'\cdot p\cdot(p^2+q^2)^2\cdot2^5\cdot r^5}{(a'^2+r^2)^{\frac{13}{2}}} + \&c \quad . \quad . \quad . \end{aligned}\right\} (c)$$

where the law of the development is evident.

In the foregoing expression p and q have the following meaning:—

$$r\,p = \alpha\,x - \beta\,y \qquad x = a\,.\,(\cos u - e)$$
$$r\,q = -\gamma\,x + \delta\,y \qquad y = a\,.\,\sqrt{1-e^2}\,.\,\sin u.$$
$$\alpha = \cos\omega'\,.\,\cos\omega + \sin\omega\,.\,\sin\omega'\,.\,\cos\iota,$$
$$\beta = \cos\omega\,.\,\cos\omega' - \cos\omega\,.\,\cos\omega'\,.\,\cos\iota,$$
$$\gamma = \sin\omega'\,.\,\cos\omega - \cos\omega'\,.\,\sin\omega\,.\,\cos\iota,$$
$$\delta = \sin\omega\,.\,\sin\omega' + \cos\omega\,.\,\cos\omega'\,.\,\cos\iota,$$

where, as before, ι denotes the inclination of the comet's to the planet's orbit, and ω and ω' the distance of the respective orbits' the perihelia from comet's ascending node in the planet's orbit.

In order to obtain the secular variations of the elements we must differentiate R_1 with respect to ω, ω', e, and ι, and then integrate the result so obtained with respect to the time t. Thus, for example,

$$\delta e = -\frac{a\,.\,n}{e}\,.\,\sqrt{1-e^2}\,.\int(A_0 + A_1\,.\cos u + A_2\,.\cos 2u + \&c. \ldots)\,.\,dt$$

where

$$A_0 = \frac{1}{2\,.\,\pi}\,.\int_0^{2\pi}\frac{dR_1}{d\omega}\,.\,du, \qquad A_2 = \frac{1}{2\,.\,\pi}\,.\int_0^{2\pi}\frac{dR_1}{d\omega}\,.\cos u\,.\,du, \ \&c.$$

These last integrals are obtained by the application of elliptic functions. For putting

$$r = a\,.\,(1 - e\,.\cos u), \text{ and } (\tan\tfrac{1}{2}\,.\,u)^2 = \frac{\alpha}{\beta}\,.\,(\tan\tfrac{1}{2}\,.\,\psi)^2,$$

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where $\alpha^2 = \frac{a'^2 + a^2 \cdot (1-e)^2}{a'^2 + a^2}$, and $\beta^2 = \frac{a'^2 + a^2 \cdot (1+e)^2}{a'^2 + a^2}$,

we get

$$\int_0^{2\pi^*} \frac{du}{(a'^2+r^2)^{\frac{2n+1}{2}}} = \frac{k}{\sqrt{\alpha \cdot \beta} \cdot (a'^2+a^2)^{\frac{2n+1}{2}}} \cdot \int_0^{2\pi} \frac{(1+\mu \cdot \cos\psi)}{\{1-e^2 \cdot (\sin\psi)^2\}^{\frac{2n+1}{2}}} \cdot d\psi \ldots (d)$$

where $k = \frac{\alpha+\beta}{2\alpha\beta}$, $\mu = \frac{\beta-\alpha}{\beta+\alpha}$, $e^2 = \frac{(\alpha+\beta)^2-4}{4\alpha\beta}$.

We have, moreover,—

$$\cos u = \frac{\mu + \cos\psi}{1+\mu\cos\psi} \quad \text{and} \quad \sin u = \frac{\sqrt{1-\mu^2} \cdot \sin\psi}{1+\mu\cos\psi}$$

$$\text{and if} \quad 1+e\cdot\mu = f \quad \text{and} \quad e-\mu = g,$$

$$1 - e\cos u = \frac{f+g\cdot\cos\psi}{1+\mu\cdot\cos\psi}; \qquad \cos u - e = \frac{f\cdot\cos\psi - g}{1+\mu\cdot\cos\psi}$$

The integral, in the right-hand member of equation (d), may, as is known, be expressed by elliptic functions. Furthermore, since the value of $\frac{d R_1}{d \omega}$, and the other differentials of R_1, are composed of terms, all having the form,—

$$\frac{B}{(a'^2+r^2)^{\frac{2n+1}{2}}},$$

in which

$$B = f(\cos u, \sin u, a, e)$$

it is easily seen that they may all be expressed by elliptic functions.

To the numerical application of the foregoing, I shall have the honour of returning on a future occasion; and for the present, therefore, confine myself to the following remarks.

In the value of the mean longitude, a term, varying as the square of the time, can arise only from the perturbations of the second order, which belong to the epoch. For if in the perturbations of the epoch of the first order, which vary proportionally to the time, we insert the variations of the elements, which are also proportional to the time, there must necessarily arise a term, varying as the square of the time.

I find, by an approximate calculation, that the variation of the eccentricity of Encke's comet, as far as it is caused by the influence of the asteroids, must, for the present, be negative; so that

$$\delta e = -t \times \text{constant}.$$

There arises, mainly through the action of *Jupiter* and *Saturn*, in the value of the epoch, a term proportional to the time, to be

* Mr. Wackerbarth, in a note which we regret we have not room to insert here, has given the analysis of the process by which this transformation is effected.—Editor.

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added to the comet's mean anomaly $n\,t$. If in this the value of $\partial\, e$ be inserted, there must result a term diminishing with the square of the time. Whether, however, that term be of sufficient amount to account for the observed circumstances in the comet's period, is another question, which mainly depends upon the value we assign to the asteroids' masses and the eccentricity of the assumed common orbit. Against this, it may be urged, that observation does not indicate any very considerable change in the eccentricity of the comet's orbit; and, moreover, that the value of the asteroids' masses cannot unconditionally be assumed greater than what answers to their influence on the nearest planets.* But, on the other hand, it is not necessary to ascribe to that cause alone the whole of the observed diminution of the period, since that can partially be explained by the perturbations of the first order, which the asteroids cause to the period or epoch; and besides, the elements of the orbit themselves must be somewhat altered if the ether's resistance be ignored. The necessity of a correction for the period of Encke's comet may, indeed, be considered as fully established by observation, but not so the form of that correction. For as far as that proof rests upon the apparitions of 1805; 1795, and 1786, the observations are better satisfied by assuming a periodic correction, embracing about seventy-two revolutions of the comet; though, it must be owned, that this does not in any way facilitate the explanation of the phenomenon.

In the foregoing investigation, I have assumed that the resisting medium is the same ether, whose existence is presupposed as necessary for explaining the phenomena of light and heat: one might also suppose that the resisting medium is the last stratum of the sun's atmosphere, which, by its reflexion, probably causes the so-called "corona" in a total eclipse. It happens, however, in this case, that that atmosphere must participate in the sun's rotation, and thus its resistance must be insensible.

* Since the above was written, the Author has had the opportunity of examining, in the *Comptes Rendus*, Nov. 28, 1853, an interesting article, by M. Le Verrier, on the masses of the small planets. Le Verrier considers that, with regard to the relative positions of the common asteroidal orbit, and *Mars'* orbit, the second term of the series, expressing the motion of *Mars'* perihelion, rises to $\frac{1}{4}$ of the first term of the same series. If, then, under this supposition, we take the aggregate mass of all the small planets as equal to the earth's mass, they must, according to M. Le Verrier, cause an inequality in the heliocentric longitude of *Mars'* perihelion, amounting to 11′ in a century. Now, as an inequality of one fourth of that amount could not assuredly have escaped the notice of observers, M. Le Verrier concludes that the sum of the masses of the small planets cannot surpass $\frac{1}{4}$ of the earth's mass. Even if with time this fraction should undergo some considerable modification, still a possibility is thus afforded for determining the limit of the masses of the small planets, which cannot but be of the highest utility in the question of the influence of these masses on Encke's comet.

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The following communication relative to the elements of Comet II. 1854, has been received from Mr. Powell. It will be seen that it includes the application of the corrections for aberration and parallax, which were not taken into account in his previous investigations.

Correction of the Elements of Comet II. 1854.
By E. B. Powell, Esq.

Greenwich M.T. April	Long. Observed, Corr. for Aberr. and Parallax.	Corr. Applied.	Lat. Observed, Corr. for Aberr. and Parallax. (South)	Corr. Applied.	
	° ′ ″		° ′ ″		
8·06067	49 51 43	+47	5 22 57	+33	The sun's real Long. was used, instead of that in the *Nautical Almanac*.
13·087693	59 0 44	+41	12 33 45	+26	
27·08739	75 31 8	+31	24 29 4	+16	

"The aberration was obtained from the diurnal motion and distance of the comet by means of a table in Vince's Astronomy; and the parallax was found roughly by taking the height and longitude of the nonagesimal from a large celestial globe, and then applying calculation.

"On pushing further the approximations, I found

D, the perihelion distance = ·2773579
E, the time of perihelion passage = March 24·010625

which afforded the errors of anomaly,

$u - v$ = + 1″, from 8th to 13th, in an angle of 10° 41′
$u' - v'$ = −16″, from 8th to 27th, in an angle of 27 11

"If D be diminished by ·00003, the errors become

$$u - v = +6'' \quad \text{and} \quad u' - v' = -6''$$

and therefore less on the whole; but I have no doubt of the observation of the 27th being more inaccurate than those of the 8th and 13th; and, consequently, I am inclined to prefer the value of D first mentioned.

"The resulting elements are

D = ·2773579
E = March 24th, ·010625, or 24th, $0^h\ 15^m\cdot3$
Inclination of Orbit = 82 26 43
Long. ☊ = 315 30 11
Long. ϖ on ecliptic = 167 56 19
Motion retrograde.

"Working out the comet's place on the 18th,

The error of long. = about 10′
The error of lat. = about 4

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The very great obliquity of the orbit affects the accuracy of the determination of the inclination, the angle in the numerator of the expression,

$$\cos i = \frac{\sin(\varphi - \varphi')}{\sin(v' - v) \cdot \sec \lambda \cdot \sec \lambda'}$$

being only 4° 19′ 13″, using the interval from the 8th to the 27th.

"It may be remarked that Pontécoulant's modification of La Place's method of approximating to the two fundamental elements gives an error of about −·0056 in the perihelion distance, and one of about −·0078 days, or −11 minutes, in the time of perihelion passage.

"*June 5th*, 1854."

Observations on an important Phenomenon observed with regard to the Hill of Santa Lucia, situated in the City of Santiago de Chile. By Don Carlos Moesta.

"The summit of the hill of Santa Lucia, situated in the capital of the Republic of Chili, is 630 mètres above the level of the sea, and about 60 above the city. Its greatest base is from N. to S., measuring some 5 cuadras (the cuadra is 150 varas square); its perpendicular line scarcely 2 cuadras.

"The rock of the hill, at first sight, appears to be a basalt; but, on examination, Señor Domeyko has determined it to be a variety of metamorphic porphyry, which he calls 'porfido abijarrado.' The columns of the rock have in various parts not only different directions, but various inclinations. However, the columns in the most southern part of the hill run W. 60° N. with an inclination of 36° to S.E.; the columns at the N. are nearly horizontal, with direction to W. a little to N. The upper part of the hill N.E. and S. is covered with a layer of earth, and not much vegetation; part of it also has broken stones (angular); but to the W., where there is a quarry, the heads of the columns are seen, and it is here where the hill is very steep. It has been necessary to enter into the above details to better understand the phenomenon I am about to refer to.

"In the N. part of the hill was erected, in 1849, the Astronomical Observatory, and it was then believed this was the most favourable position. I here only allude to the house which contains the transit instrument. The axis of the instrument rests on two blocks of stone of $6\frac{1}{2}$ feet, which are placed in solid masonry upon the firm rock, without touching any broken or soft parts of the rock.

"The rock that serves as foundation to this masonry, as I have already stated, is of the before-mentioned columns of porphyry; the direction of these is about in a line with the axis of the instrument, and with but little inclination. In the construction of the instrument, as well as in the fixing it, the greatest care

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was taken, so as to compensate for all exterior influences of variation of temperature, pressure of the air, &c.

"Independently of all this, Mr. Gilliss* has observed from the period of the erection of the observatory, that the end of the axis to the E. was constantly elevating itself, and that it was found necessary to lower this end from time to time, so that the instrument might be maintained in the most convenient position for observing. This phenomenon has been observed since then until the present time, and clearly shows that the block on which rests the extremity of the axis to the east is being raised above the surface of the earth, and in such a manner, that if we add the small quantities, the other end has been lowered since the erection of the observatory in 1849 (these observations of Moesta's were made in March 1853), the difference will be *a quarter of an inch.*

"So interesting a phenomenon induced me to examine if this elevation was effected suddenly, that is to say, by movements caused by earthquakes, which are scarcely sensible on the surface of the earth, or by a motion slow but gradual.

"I proposed to level the instrument every 12 hours, with all possible exactitude; and in carrying on these operations, I observed another phenomenon, not less interesting than the first. I observed that in taking these levels, with the large level of the transit, the position of the axis was subject to oscillations dependent on temperature.

"Here are the observations I refer to:—

Days. 1853.	Height of Axis to the East.	Height of Axis to West.	Error of Obs.	Prob. Error of Result.	Temperature, Fahr. °	State of Atmosphere.	
Mar. 4	0·007	...	0·190	0·064	70·7	Clear and calm.	
5	...	1·049	0·246	0·058	91·6	—	—
	0·063	...	0·037	0·009	70·0	—	—
8	...	1·794	0·074	0·022	84·0	—	South wind.
	...	1·052	0·206	0·069	62·5	—	Calm.
9	...	2·227	0·067	0·023	89·0	—	—
	...	1·210	0·121	0·040	69·7	—	—
11	...	2·431	0·220	0·065	88·5	Cloudy	—
	...	1·803	0·240	0·064	68·0	Clear	—
12	...	2·369	0·204	0·061	84·5	Cloudy;	strong S. wind.
14	...	2·695	0·050	0·016	68·0	—	Calm.
	...	2·475	0·029	0·009	62·5	—	—
15	..	3·041	0·151	0·045	77·5	Clear	—

"This table contains, independently of the difference of elevation of the two extremities of the axis, the temperature and state of the atmosphere; and so that we may have an idea of the exactitude of the observations, I have calculated the medium

* Chief of the United States Astronomical Expedition.

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errors of the observations, as well as the probable. Observe that the observations are made at intervals of 12 hours. It is sufficient to look at the table to be convinced that the end of the axis to the E. rises and falls with elevation or depression of temperature; and as, by the construction of the instrument, the temperature influences both ends of the axis in a similar manner, we have to look for the cause of that oscillation away from the instrument; *this cause can be no other than the expansion and contraction of the columns of porphyry*, in which is placed the block that supports the west extremity of the axis; expansion and contraction caused by the heat of the sun during the day and the cold at night.

"Thus, for example, on the 4th and 5th March, the axis rose to the west,

1″·056

whilst the temperature rose

70°·7 to 91°·6 F.

"The same axis fell afterwards

1″·112

"Temperature depressed

91°·6 to 70° F.

"Little more or less we find this same result in the second group of observations; and if the difference of level in these two groups have not the same relation, with the corresponding differences of temperature, less have we of coincidence in the following group. The 11th and 12th of March the difference of level was only

	0″·628
Temperature depressed from	88°·5 to 61° F.
And it is	0″·596
Temperature rising	68° to 84°·5 F.

"But this inequality of the differences of level for equal differences of temperature is a proof most truly, that the expansion of the rock produced by the sun's heat is the cause of the rising of the hill.

"The first two groups of observations show that the difference of level was a little more or less than

1″

the difference of temperature rising, medium height, to 20°; whilst in the third group the difference of level is only

0″·628,

notwithstanding the difference of temperature was the same.

"But the last column of the table indicates that at the same time, during the first observations, the sky was clear, and cloudy

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during the others. It is evident that, with a clouded sky, the hill or rock cannot become so heated as when it receives the direct rays of the sun; and, consequently, the difference of the temperature of the air and that of the rock must be less in the first case than in the second.

"The temperature of the rock will also depend upon the wind; for example, it will be less when blowing strong from S., or when it is calm. Supposing in both cases the temperature of the air to be the same. Thus, then, there cannot be a constant relation between the differences of level and the difference of temperatures; but it is more than probable that we shall find determined the differences of the temperature of the same rock, in place of the temperature of the air.

"The last group of the observations presents an example in which the level, during 12 hours, remains constant: this fact coincides with the very small change of temperature; in effect, the temperature for the difference being (on the 14th) only 5°·5; the difference of level was not above

0″·22

"According to these obserrvations, there cannot be a doubt that the heat of the sun is the cause of the phenomenon in question; and now it only remains to inquire, how comes it that there is only a partial rising of that part of the hill situated to the west, which elevation is indicated by the level.

"To resolve this question, we must again revert to the topography and geology of the Hill of Santa Lucia.

"I have said that in the W. part of the hill the porphyritic columns are exposed to the immediate influence of the sun, whilst in other parts the hill is covered with a layer of vegetation and broken rock, which protects the said columns from the sun's heat. Moreover, these columns have a N.W. direction, with a strong inclination to the west; so that being exposed from 12 P.M. to the evening to the sun's rays, which fall almost perpendicularly upon the heads of the columns, will necessarily suffer a greater expansion than the great body of the hill situated to the N. and E., which is shaded.

"And thus we must be struck with this wonderful power or force of the sun which causes to rise and fall *periodically* this enormous mass of firm and hard rock analogous to that other force which causes to rise and fall *periodically* the column of mercury in the barometer.

"It will now easily be conceived that in the vicinity of Santiago there could not be a worse place for an observatory than the Hill of Santa Lucia, it being of the greatest importance to have such established at a point where the atmospheric influences have the least possible effect on the instruments."

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On the Phenomenon seen during the Total Eclipse of the Sun of November 30, 1853.

(*Extract of a Letter from Capt. Shea.*)

"On perusing the account of the total eclipse of the sun on the 30th November, 1853, at Ocucaja, in Peru (communicated by Admiral W. H. Smyth), in No. 8, June 9th, 1854, of the Royal Astronomical Society's *Monthly Notice*, I find, on referring to my register of the spots passing over the sun's disc, that the spot seen here on the 24th of November, 1853, would occupy the same position in the solar hemisphere as the 'rose-coloured protuberance' described in the account above referred to."

Captain Shea further remarks, that the observations of the total eclipse of the sun, July 28, 1851, appear to him to strengthen the opinion of the identity of the solar spots with the rose-coloured protuberances.

Tables for Facilitating the Determination of the Latitude and Time at Sea by Observations of the Stars. By Charles F. A. Shadwell, Esq., F.R.A.S., Captain Royal Navy. New edition, 8vo. London, 1854.

This new edition of a work well known to the naval profession is distinguished by various improvements, which have a tendency to promote its useful character. Several additional couplets of stars are inserted in Tables I. and II., which, it may be stated by way of explanation to those who have not seen the work, are computed with a view to abbreviate the process for finding the latitude by "Simultaneous Altitudes," or, in other words, by simultaneous observations of the altitudes of two stars.

The author has pointed out the facility with which, under certain favourable circumstances, the same method may be employed for finding the Time, and he has computed a new Table (Table III.) with a view to the promotion of this object. He has also computed a fourth Table adapted to the method "for finding the latitude by the combined altitudes of two stars taken at an interval of time equal to the difference of their right ascensions."

In reference to the last-mentioned method, which was originally given in Lynn's Navigation, but appears to have subsequently fallen into oblivion, the author remarks that, "since the observations required are in themselves very simple, since the practical fulfilment of the requisite condition as to time demanded by the problem is not attended with any particular difficulty, and since the actual computation may be materially facilitated by the use of previously prepared constants depending on the declinations of the stars, there does not seem to be any valid objection to the revival of this problem, and to again submitting it to the notice of the scientific navigator. Moreover, since the period during which the observations may be

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made is not restricted to any precise moment, as in the case of meridional altitudes, advantage may frequently be taken of the favourable circumstances of morning and evening twilights, when the distinct definition of the sea-horizon is conducive to the accuracy of the results; a recommendation analogous to that which attends the method of Simultaneous Altitudes."

Leçons de Cosmographie, Rédigées d'après les Programmes Officiels. Par H. Faye, Membre de l'Institut, &c. &c. Deuxième edition. 8vo. Paris, 1854.

This work contains a lucid exposition of the more elementary principles of astronomy adapted to the present state of the science. The early appearance of a second edition is a sufficient proof of the esteem in which the work is held in France.

Sitzungsberichte der Kaizerlichen Akademie der Wissenschaften. Band XII. (Jänner.) Wien, 1854.

This publication contains three astronomical papers, which it may be occasionally desirable to consult. The first of these is by M. Hornstein: it contains a determination of the orbit of Comet I. 1853, founded upon the totality of the observations of the comet, extending from March 6 to April 11. The second is by M. Littrow, and refers to the points of resemblance which subsist between the orbits of the various bodies composing the solar system. The third, by M. Oeltzen, is devoted to a comparison of the Zone observations of Bessel and Argelander.

Astronomical Observations made at the Radcliffe Observatory in the year 1852. By Manuel J. Johnson, M.A., Radcliffe Observer, Vol. XIII. Oxford, 1854.

The Meridian Observations in this volume, with few exceptions, relate to the Circumpolar Catalogue. The author states that another year will be required to complete the latter on the system laid down of not admitting any star which has not been observed in at least two different years. "This system," he remarks, "though it has prolonged our task, has been found so useful a check against errors and confusions of all kinds, as to afford ample compensation for delay." The author announces that an investigation of the parallaxes of 61 *Cygni* and 1830 *Groombridge*, founded upon observations with the heliometer, will appear in the next volume, which will be shortly published.

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Informe sobre las Observaciones hechas durante el Eclipse Solar de 30 Noviembre de 1853, presentado al Senor Ministro de Instruccion Publica. Por Carlos Moesta. Santiago de Chile, 1854.

This account of the total eclipse of the sun of November 30, 1853, does not differ from that communicated to the Society in the course of last summer by Admiral Smyth, and of which a translation was given in the *Monthly Notice* for June 1854 (Vol. XIV., p. 225), except that it is accompanied by a drawing of the appearance of the eclipse which some persons may be desirous of inspecting. It may be remarked, however, that the details of the drawing exhibit a satisfactory agreement with the description given in the text.

Astronomische Waarnemingen gedaan ter bepaling der geografische ligging van Batavia door S. H. De Lange en G. A. De Lange, geografische Ingenieurs von Nederlandsch Indië.

(*Communicated by the Astronomer Royal.*)

This Paper, as the title imports, contains the details of a series of observations instituted with a view to determine the longitude of Batavia. These observations consist chiefly of determinations of the zenith-distance of the moon, and of the right ascensions of the moon and moon-culminating stars. They were commenced in 1851, but were interrupted by the unfavourable state of the weather in the month of October in that year, and soon afterwards by the return of MM. De Lange to Manado. They were, however, resumed in May 1853, and continued to be prosecuted pretty regularly till towards the close of the same year.

The observatory consisted of a hut built of bamboos for the occasion, which could be opened with facility in every direction. The instruments with which the observations were made were a universal instrument by Pistor and Martin of Berlin, and a similar instrument by Repsold.

It was found inconvenient to erect a meridian mark; a bamboo was merely fixed in the direction of W.S.W. at a distance of 200 yards from the place of observation; its azimuthal deviation from the meridian having been determined with the utmost possible accuracy. In determining the error of collimation, the method of making one of the telescopes serve as a collimator to the other was very frequently employed. The adjustment of the telescopes upon each other was very conveniently effected.

In the focus of the telescope two vertical wires were placed very near to each other, which could be very accurately brought upon the wires of the telescope employed as the collimator.

In order that an opinion of the value of the observations may

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be formed, a comparison is instituted between the results for right ascension obtained by the two observers on every night of the moon being observed independently by each of them. The following are the outstanding differences:—

s	s
−0·06	+0·13
−0·17	+0·03
+0·06	−0·42
−0·22	+0·56
+0·04	−0·24
−0·29	+0·05
−0·09	−0·16
+0·01	−0·05
−0·05	+0·10
+0·07	−0·55
	−0·28

The author estimates the probable error of an observation of right ascension to be 0ˢ·92, a quantity which, however, he is inclined to think exceeds rather than falls short of the true value.

"The author then proceeds to give an account of the operations of himself and his colleague for determining the longitude from the observed zenith-distances of the moon. The method pursued by them in this instance was suggested by Kaiser, who remarks that it is only applicable in practice when the moon's latitude is small, since the variation in right ascension then takes place almost wholly in altitude.

"In the foci of both instruments," says the author, "I inserted two additional wires on each side of the double horizontal wires which had been already introduced. The instrument being adjusted with all possible precision, the telescope was directed to the moon, when the time of the passage of her limb over each of the horizontal wires was accurately noted and the level of the vertical circle read off. By the aid of a slight previous calculation, it was easy to find the instant of time (and also the corresponding azimuth) when a star in the vicinity of the moon should attain the same altitude. The instrument was then placed in the known azimuth (the vertical circle retaining a fixed position), and the star being brought into the field of view of the telescope by its diurnal motion, the time of its passage over each of the horizontal wires was observed and the level read off. The altitude of the star for the time of observation may be accurately found by calculation, and it is manifest that the apparent zenith-distance of the moon's limb will be equal to the computed zenith-distance of the star, subject to a small correction for the variation of the level corresponding to the interval between the two observations. The following are the advantages of this method:—

"1°. A small error in the determination of the time does not exercise any sensible influence.

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"2°. The effect of refraction is eliminated.

"3°. The method is independent of the unavoidable errors which the observation of zenith-distances entails upon the reading off of the vertical circle. It is also independent of the errors of the instrument, as well as those of the micrometer screws, and of the error of collimation.

"4°. The observation may be repeated several times."

The following process was employed for deducing the longitude from the observations:—

With an assumed longitude ($7^h\ 7^m\ 37^s$ East) the Greenwich mean time of the observation of the moon's limb was found, and the corresponding right ascension and declination of the moon calculated from the *Nautical Almanac*.

From these data the zenith-distance of the moon's centre was finally calculated, which, if the assumed longitude was correct, ought to agree with the observed zenith-distance. If no such agreement was found to subsist, the difference of the two quantities served to indicate the difference of longitude which would result by repeating the calculation with a longitude greater or less by 10^s than the longitude assumed. The author remarks that the agreement of the results obtained in the same night by this method leaves nothing to be desired, and that the probable error of an observed zenith-distance does not amount to more than $1''{\cdot}3$, which on an average agrees with an error of $2^s{\cdot}6$ in the longitude deduced from it, a degree of accuracy, he adds, which is not to be obtained by any other method with so small instruments.

The results of the observations are then given. The longitude for each observed zenith-distance is computed, and also the small difference which would result if the assumed longitude were increased by 10^s. The transit-observations next follow with the individual values of the longitude deduced from them. The Paper concludes with the equations of condition for the correction of the longitude based upon a few observed occultations. The method of calculation employed is that given by Professor Challis in the *Nautical Almanac* for 1854.

Telescopic Appearance of the Planet Venus at the time of her Inferior Conjunction, February $28^d\ 1^h\ 45^m$, 1854. By John Drew, Ph. D.

"Having ascertained from the *Nautical Almanac* that on this occasion the heliocentric latitude of *Venus* was nearly at its maximum, I determined to watch her if possible at the time of conjunction; and, as the sky was particularly clear, I succeeded in taking observations of the planet both before and after the occurrence.

"Measurement with the position wire micrometer gave, as nearly as it would admit, $3''$ as the breadth of the illumination: more than half of her circumference was visible, but its extent I

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regret that I did not ascertain. On a similar occasion (May 1849) the distance from one cusp to the other was found by M. Mädler, making use of the Dorpat refractor, to be no less than 240° (*Astronomische Nachrichten*, No. 679), whence he deduces 43′ 7″ as the amount of horizontal refraction in *Venus*, or greater than that of our atmosphere by about one-sixth.

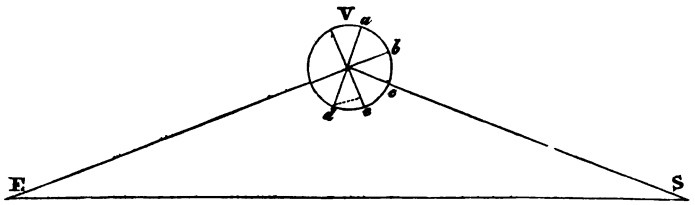
"With the object of ascertaining how much of the breadth of the crescent was due to refraction, I calculated the breadth of that portion of her surface which only would be visible, supposing the planet was not surrounded by an atmosphere, and found it to be 0″·67. The difference between this and 3″ is, I apprehend, caused by the refraction of the planet's atmosphere.

"I subjoin the calculation and a drawing of the appearance of the planet at the instant of her inferior conjunction.

	°	′	″
Heliocentric latitude of Venus	3	22	32
Geocentric latitude	8	48	10
Illuminated portion = the sum	12	10	42

"The versed sine of this last quantity is ·0225: hence ·01125 of her diameter (59″·6) = 0″·67 will be the breadth at the widest part of the illuminated crescent.

"The observations were taken at my observatory, Southampton, with a 5-foot refractor by Dollond.



"Let V be *Venus;* E the Earth; S the Sun; *a d* the boundary of light and darkness; *d e* the illuminated portion, as seen from E = *b c*; but *b c* = ∠ E + ∠ S = geocentric lat. + heliocentric lat."

Contribution to the Theory of Elliptic Functions. By Carl John Malmsten, Professor of Mathematics in the University of Uppsala.*

In this paper the author gives a new demonstration of the three well-known relations connecting the elliptic function of the third order

$$\Pi(n, k, \varphi) = \int_0^{\varphi} \frac{d\varphi}{(1 + n \sin^2 \varphi)\, \Delta(k, \varphi)}$$

* Communicated by A. D. Wackerbarth, Esq.

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with those of the first and second order

$$F(k, \varphi) = \int_0^{\varphi} \frac{d\varphi}{\Delta(k, \varphi)}$$

$$E(k, \varphi) = \int_0^{\varphi} \Delta(k, \varphi)\, d\varphi$$

The author remarks that Lagrange established the relations between these functions by a process mainly founded on the differentiation of the parameter n; but he shows that the same object may be effected, independently of the differentiation of n, by means of the differential equation of the second order which is satisfied by $F(k, \varphi)$ and $E(k, \varphi)$.

Elements of Euphrosyne. By M. Winnecke.

Epoch, Sept. 1·0, M. T. Berlin.

M	=	299° 0′ 3″·4	
ϖ	=	95 13 45·1	Mean Equinox,
Ω	=	31 11 59·9	1855·0
i	=	26 53 26·0	
φ	=	13 15 46·2	
μ	=	622·091	
Log a	=	0·504102	

These elements are calculated from the Washington observation of September 2, and the Berlin observations of October 1 and 31.

Elements of Pomona. By M. Bruhns.

Epoch, 1854, Nov. 0·0, M. T. Berlin.

M	=	206° 32′ 27″·6	
ϖ	=	195 46 56·0	Mean Equinox,
Ω	=	220 44 20·5	1855·0
i	=	5 39 2·9	
φ	=	5 29 27·6	
μ	=	853·694	
Log a	=	0·412470	

These elements are calculated from the Paris observation of October 28, and the Berlin observations of November 6 and 14.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *January* 12, 1855. No. 3.

Dr. LEE, V.P., in the Chair.

John Saunders Muir, Esq., 15 Harrington Square;
R. Hartley Kennedy, Esq, Kennington Lodge;
George Hamilton, Esq., Egremont, Cheshire; and
William Tomlinson, Esq., Grove Lodge, York,

were balloted for and duly elected Fellows of the Society.

Dr. Brünnow, Ann Arbor, Michigan;
Lieut. M. F. Maury, U.S.N., Director of the National Observatory, Washington; and
Dr. Benj. Althorpe Gould, Cambridge, Massachussets,

were balloted for and duly elected Associates of the Society.

Professor Hansen has announced in a letter to the Astronomer Royal that his Lunar Tables are steadily advancing towards completion. The Lords of the Admiralty have assigned to him an additional grant of 100*l.* towards defraying the expense of their construction.

The *Astronomical Journal*, edited by Dr. B. A. Gould, may be obtained of Messrs. Trübner and Co., 12 Paternoster Row, who have been appointed agents for the sale of that publication in England. It is published at irregular intervals in numbers twenty-four of which form a volume. The price of each volume is 25*s.*

Extract of a Letter from Thomas Maclear, Esq. on the Observations of Schweizer's Comet (Comet II. 1853).

"I hasten to submit to the Royal Astronomical Society with the least practicable delay the accompanying REVISAL copy of the observations made here on Schweizer's Comet.

"On receiving last Tuesday the Society's Notice, No. 8, vol. xiv., which contains Professor Challis's determination of the mean places of the stars compared with the comet, I referred to Notice No. 1 of the same volume to collate the approximate places

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sent from hence, when the accidental sight of an expression detected an oversight in the reduction of a considerable portion of the comet comparisons,—viz. the measures on the parallel for differences in right ascension by means of the micrometer screw (marked by the letter *d* in column 8) were inadvertently multiplied instead of divided by *sine north polar distance;* and the signs of those of May 20, 21, and June 4, should be changed.

"The present paper represents the observations corrected for the effect of refraction; and I rather hope than expect that it may be received in time to prevent any use being made of the former communication for the elements of the orbit. Moreover, I shall exceedingly regret if the oversight above mentioned should impair the confidence these observations deserve.

THOMAS MACLEAR.

"P.S. Referring to the original register of the observations for the approximate place of star No. 21, where there is a diagram of the field of the telescope, there must have been an error in copying, for the difference in R.A. is there entered + 4ˢ, and in polar distance — 11′ 17″.

"On referring this week to the heavens, I find by micrometric measurement, corrected for refraction, + 4ˢ·455 and — 11′ 23″·18. Applying these numbers to the mean place of star 20 given by Professor Challis, the R.A. of star 21 is 7ʰ 11ᵐ 19ˢ·33, N.P.D. 103° 26′ 24″·55."

Revised Copy of the Observations made on Schweizer's Comet at the Cape of Good Hope. By Thomas Maclear, Esq.

The observations are corrected for refraction only.

The letters *t* and *d* in column 8 denote respectively the right-ascension differences obtained by transits, or by the subtense of the spider lines as given by the micrometer scale.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 1	7 46 28·10	...	...	+2 50·24	2	1	
	7 56 37·29	−1 46·532	2	...	...	1	*t*
	8 6 17·36	...	...	+3 38·17	2	1	
	8 26 37·32	...	...	+4 25·86	5	1	
	8 37 17·28	−0 34·479	1	...	...	1	*t*
3	7 19 49·87	...	...	+5 12·82	3	2	
	7 42 23·62	−1 12·658	4	...	...	2	*t*
	7 42 23·62	−1 34·739	4	...	...	3	*t*
	7 42 23·62	−1 53·530	4	...	...	4	*t*
	7 56 28·52	...	...	+5 25·34	3	2	
	8 10 29·46	−0 50·447	3	...	...	2	

Clouded on the 2d.

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1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 3	9 11 36·35	−0 1·623	8	...	...	2	t
	9 18 32·71	...	...	+ 5 41·45	1	2	
4	6 35 11·59	...	...	− 4 56·02	2	5	
	6 57 18·62	+0 34·348	9	...	...	5	t
	6 57 18·62	−0 17·970	9	...	...	6	t
	7 14 42·60	...	...	− 3 19·55	2	6	
	7 22 54·73	−0 3·370	9	...	...	6	t
	7 35 59·60	...	...	− 3 20·31	2	6	
	8 8 11·72	+0 25·340	5	...	...	6	t
	8 30 21·54	...	...	− 3 19·85	3	6	
	8 44 49·83	+0 44·486	3	...	...	6	t
	8 10 53·45	−1 22·002	3	...	...	7	t
	8 30 21·54	...	...	− 1 4·42	3	7	
	8 50 28·63	−0 58·841	6	...	...	7	t
	9 13 5·16	...	...	− 1 7·67	2	7	
5	7 0 27·44	...	...	+11 28·42	1	8	
	7 37 17·49	−7 29·912	1	...	...	8	t
7	7 3 43·40	+1 14·379	4	...	...	9	t
	7 19 37·18	...	...	−10 13·21	2	9	
	7 36 1·49	+1 22·408	3	...	...	9	t
	8 3 37·29	...	...	−10 23·67	3	9	
	9 3 57·54	...	...	−10 31·27	2	9	
	9 11 16·84	+1 47·781	1	...	...	9	t
	7 3 43·40	−0 46·079	4	...	...	10	t
	7 25 34·21	...	...	− 7 36·70	2	10	
	7 36 1·49	−0 37·838	3	...	...	10	t
	8 35 1·80	...	...	− 7 52·92	2	10	
	9 11 16·84	−0 12·661	1	...	...	10	t
8	6 23 36·26	−0 34·350	4	...	...	11	t
	6 44 36·46	...	...	− 1 28·88	2	11	
	6 23 36·26	−1 1·502	4	...	...	12	t
	6 50 2·67	...	...	+ 3 59·82	2	12	
	6 23 36·26	−2 20·278	4	...	...	13	t
	7 0 22·20	...	...	+10 54·81	3	13	
9	7 2 3·47	+2 31·153	2	...	...	13	t
	7 30 53·78	+2 36·305	1	...	...	13	t
	7 43 20·78	...	...	+ 6 47·17	3	13	
	8 2 31·62	+2 42·029	5	...	...	13	t
	8 19 33·00	...	...	+ 6 39·63	2	13	

May 5. Owing to clouds these observations only could be obtained.

7. Angle of position of comet's tail, 118° 40′. No. 10 is in the comet's tail.

9. Angle of position of comet's tail, 121° 30′. Nucleus remarkably bright.

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1853.	Cape Mean Time. h m s	Difference in R.A. m s	No. of Obs. in R.A.	Difference in N.P.D. ′ ″	No. of Obs. in N.P.D.	No. of Star.	
May 10	7 11 44·27	...	...	−3 6·40	2	14	
	7 30 39·76	−0 27·602	1	...	...	14	*t*
11	6 36 25·27	+0 44·368	3	...	...	15	*t*
	7 0 34·48	...	...	+5 37·06	4	15	
	7 21 27·38	+0 50·129	4	...	...	15	*t*
	8 12 59·09	...	...	+5 20·65	4	15	
	8 32 5·06	+0 58·440	4	...	...	15	*t*
	6 36 25·27	−1 37·933	3	...	...	16	*t*
	7 0 34·48	...	...	+1 2·07	4	16	
	7 21 27·38	−1 32·201	4	...	...	16	*t*
	8 12 59·09	...	...	+0 45·13	4	16	
	8 32 5·06	−1 23·953	4	...	...	16	*t*
12	6 41 44·35	...	...	+1 20·44	6	17	
	6 54 57·74	−0 1·515	8	...	...	17	*d*
	7 2 55·05	−0 0·727	9	...	...	17	*d*
	7 15 7·60	...	...	+1 14·82	9	17	
	7 36 9·97	+0 2·656	14	...	...	17	*d*
	8 10 38·13	...	...	+1 5·85	5	17	
	8 19 36·27	+0 7·158	5	...	...	17	*d*
14	7 27 56·82	...	...	−5 52·53	3	17	
	8 4 32·88	+4 28·769	5	...	...	17	*t*
	8 42 50·40	...	...	−6 6·18	5	17	
15	6 32 58·31	...	...	+3 34·95	5	18	
	6 49 34·79	−0 37·701	5	...	...	18	*t*
	7 7 42·58	−0 36·625	5	...	...	18	*d*
	7 24 52·76	...	...	+3 27·48	5	18	
	7 40 2·46	−0 34·625	5	...	...	18	*d*
	7 49 22·93	...	...	+3 22·72	5	18	
	8 0 34·68	−0 33·223	5	...	...	18	*d*
	8 11 40·64	...	..	+3 20·33	5	18	
16	6 20 43·76	...	...	+7 42·41	5	19	
	6 32 35·81	−0 27·532	5	...	...	19	*d*
	6 40 49·26	...	...	+7 38·85	5	19	
	6 53 34·76	−0 26·166	5	...	...	19	*d*
	7 3 28·35	...	...	+7 37·75	5	19	
	7 14 24·55	−0 24·868	5	...	...	19	*d*
	7 21 15·63	...	...	+7 33·79	5	19	

May 10. Clouded after these observations.
11. Angle of position of comet's tail, 115° 40′. Nucleus less bright; halo round the head more diffused.
12. Angle of position of comet's tail, 117° 30′
14. ,, ,, 118 0 Clouds.
15. ,, ,, 117 35

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1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 16	7 37 17·40	−0 23·256	5	...	...	19	*d*
	7 49 10·05	...	...	+7 28·26	5	19	
	7 57 44·84	−0 22·118	5	...	...	19	*d*
	8 21 20·18	...	...	+7 23·12	5	19	
17	6 15 6·14	−0 17·635	5	...	...	20	*d*
	6 16 0·00	−0 22·284	5	...	...	21	*d*
	6 58 51·77	...	...	−7 56·53	5	20	
	7 5 4·55	...	...	+3 26·32	5	21	
	7 16 43·34	−0 14·416	5	...	...	20	*d*
	7 18 12·00	−0 19·047	5	...	...	21	*d*
	7 28 54·65	...	...	−8 0·01	5	20	
	7 34 51·87	...	...	+3 26·49	5	21	
	7 47 36·38	−0 12·801	5	...	...	20	*d*
	7 48 38·21	−0 17·328	5	...	...	21	*d*
	8 4 19·63	...	...	−8 0·36	5	20	
	8 9 58·11	...	...	+3 17·34	5	21	
20	6 42 20·46	−0 13·130	5	...	...	22	*d*
	6 49 23·50	...	...	−0 48·62	5	22	
	6 55 1·77	−0 12·631	5	...	...	22	*d*
	7 2 55·48	...	...	−0 51·64	5	22	
	7 8 59·68	−0 11·939	5	...	...	22	*d*
	7 15 43·57	...	...	−0 50·25	5	22	
	7 21 17·66	−0 11·499	5	...	...	22	*d*
21	6 41 26·83	...	...	+6 3·15	5	23	
	6 49 47·26	+0 15·169	5	...	...	23	*d*
	6 55 44·28	...	...	+6 3·05	5	23	
	7 2 29·57	+0 15·464	5	...	...	23	*d*
	7 9 28·82	...	...	+6 1·23	5	23	
23	6 35 26·93	...	...	+7 51·42	5	24	
	6 48 36·57	−0 2·862	5	...	...	24	*d*
	7 0 21·44	−0 2·726	5	...	...	24	*d*
	7 9 41·51	−0 2·275	5	...	...	24	*d*
	7 18 44·82	...	...	+7 41·29	5	24	
28	6 27 55·21	...	...	+4 55·45	5	25	
	6 45 59·48	−0 31·904	5	...	...	25	*d*
	6 56 48·34	−0 31·935	5	...	...	25	*d*
	7 6 1·61	...	...	+4 51·99	5	25	
29	6 13 20·56	−0 6·919	5	...	...	25	*d*

May 17. Angle of position of comet's tail 118° 45′.

20. The comet is faint, partly owing to the moonlight. The tail barely distinguishable.

23. Thin clouds. The comet is very indistinct.

29. The comet is very faint.

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1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 29	6 20 29·69	...	...	+2 32·25	5	25	
	6 29 17·02	−0 6·573	5	...	...	25	*d*
	6 35 10·81	...	...	+2 30·35	5	25	
	6 40 52·64	−0 6·273	5	...	...	25	*d*
31	6 24 32·60	+0 38·938	5	...	...	25	*d*
	6 31 0·12	...	...	−2 5·16	5	25	
	6 37 46·37	+0 38·917	5	...	...	25	*d*
	6 46 57·97	...	...	−2 9·65	5	25	
	6 54 6·78	+0 38·865	5	...	...	25	*d*
June 1	6 27 12·98	...	...	+10 1·96	5	26	
	6 34 57·26	−0 50·387	5	...	...	26	*d*
	6 41 52·68	...	...	+9 58·95	5	26	
	6 50 17·87	−0 49·796	5	...	...	26	*d*
	7 14 34·44	−0 48·083	3	...	...	26	*t*
	7 14 34·44	+1 0·237	3	...	...	25	*t*
	7 24 49·65	...	...	−4 23·62	5	25	
2	6 27 25·54	+1 18·965	3	...	...	25	*t*
	6 45 16·64	...	...	−6 26·94	4	25	
	7 1 22·02	+1 19·253	2	...	...	25	*t*
	6 27 25·54	−1 24·261	3	...	...	27	*t*
	6 45 16·64	...	...	−2 53·07	4	27	
	7 1 22·02	−1 23·896	2	...	...	27	*t*
3	6 36 53·39	...	...	+5 54·40	5	26	
	6 45 8·39	−0 11·989	5	...	...	26	*d*
	6 52 20·58	...	...	+5 53·80	5	26	
4	7 7 22·67	...	...	+3 53·75	1	26	
	7 13 12·69	+0 5·810	2	...	...	26	*d*
	7 25 19·64	...	...	+3 47·42	2	26	
5	6 34 48·27	...	...	+2 4·91	3	26	
	6 45 21·34	+0 22·608	5	...	...	26	*d*
	6 55 12·79	...	...	+2 4·20	3	26	
7	6 35 58·61	...	...	−1 22·38	3	26	
	6 47 3·93	+0 54·498	10	...	...	26	*d*
	6 58 43·86	...	...	−1 24·30	5	26	
	7 16 36·13	+0 54·550	10	...	...	26	*t*
	7 31 40·34	...	...	−1 26·34	2	26	
8	6 49 9·52	+1 8·199	5	...	...	26	*t*
	7 7 54·37	...	...	−2 59·49	10	26	
	7 26 44·26	+1 9·505	3	...	...	26	*t*

June 1. The measurements diminish in precision owing to the faintness of the comet.

3. The comet is very faint.

4. Generally clouded. These observations only could be obtained.

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1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
June 9	6 23 37·43	...	...	−4 22·13	5	26	
	6 38 11·42	+1 23·619	7	...	...	26	
	6 56 13·48	...	...	−4 23·55	5	26	
10	6 23 48·76	...	...	−5 47·12	5	26	
	6 39 39·45	+1 38·681	5	...	...	26	*t*
	6 54 45·86	...	...	−5 48·28	5	26	
11	6 34 14·35	...	...	+4 51·73	5	28	
	6 45 47·37	−0 23·449	5	...	...	28	*d*
	6 55 0·64	...	...	+4 50·24	5	28	

June 10. The moonlight is now diminishing the visibility of the comet.

"*Royal Observatory, Cape of Good Hope, Nov. 12th*, 1854."

On the Telescopic Appearances of Saturn with a 7½-inch Object-Glass. By the Rev. W. R. Dawes.

In the spring of last year (1854) I availed myself of an opportunity of increasing the optical means in my possession, by the purchase of a 7½-inch object-glass, having a focal length of nearly 9½ feet. It is the work of Mr. Alvan Clark, of Boston, U.S., who has long been known in that city as a most successful painter of portraits, but took to the manufacture of telescopes as an amateur. Being dissatisfied with reflectors, on which he commenced his operations, he attempted the manufacture of object-glasses; and succeeded so well, that in the autumn of 1851 he communicated to me the places of some new and very close double stars, which he had discovered with glasses whose apertures were 4¼ and 5¼ inches. In the following year he completed an object-glass of 7⅛ inches aperture for the observatory at Williams's College, which was tried at the Harvard Observatory by the Messrs. Bond, and highly approved: immediately after which he commenced one of 7½ inches aperture, intended to be retained and mounted equatoreally for his own use. At his request I sent him some extremely difficult tests, selected from Mr. Otto Struve's *Pulkova Catalogue;* several of which have a central distance of little more than half a second, and some even less. Yet of all these I soon received from the ingenious maker (who has also proved himself an acute observer) perfectly correct diagrams; together with the places of one or two extremely difficult new double stars which he had discovered with this glass. As a specimen of these, I may mention 95 *Ceti*, which is at present favourably situated for observation. Though unwilling to part with this glass, Mr. Clark consented to let me have it to try against my Munich telescope; and in March 1854 it arrived, with its tube, finder, and eye-pieces.

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Though the crown-glass has a considerable number of small bubbles, the performance of the telescope is not sensibly affected by that circumstance. In other respects the materials are good; and the figure is so excellent, and so uniform throughout the whole of the area, that its power is quite equal to anything which can be expected of the aperture; and, consequently, both in its illuminating and separating power, it is decidedly superior to my old favourite of 6¼ inches aperture. As a specimen of its light, I may mention the companion of *v Ursæ Majoris* as having been pretty steadily seen with it; and also that I have never seen *Saturn* under tolerable circumstances during the present apparition without detecting *Enceladus*, even when at or very near his conjunctions with the planet. When exterior to a tangent to the extremity of the ring, this satellite has frequently been perceived as soon as my eye was applied to the telescope. Last spring it was seen several times in strong twilight; for instance, on March 16th, 17th, and 20th, at about 7^h G. M. T. In separating power, the glass is competent to divide a sixth-magnitude star composed of two equal stars, whose central distance is 0″·6.

I have thought it proper to premise thus much respecting the performance of the telescope, that a correct idea may be formed as to the degree of dependence to be placed upon the views it has afforded me of *Saturn;* the special subject of my present communication, to which I will now proceed.

1. *The outer Ring*, A. The interior edge of this ring is decidedly its brightest part: its light rapidly fades away towards the middle, where there is a very dark, narrow, well-defined line concentric with the ring, and about one-fifth of its breadth by careful estimation. This line has been always seen when the air was in a tolerably good state, and much more readily than last year. On the 26th November, 1854, it was traced more than half way round towards the ball, and was equally well seen at both ansæ. I have recorded on 10th January, 1855, "I am surprised at the positiveness of the dark line near the middle of this ring. It was well seen with every power from 355 to 1000." This is now the fourth apparition of *Saturn* in which I have noticed this dark line, and it does not appear to me to have varied in its position on the ring, or in its breadth and depth of shade.

2. *The interior bright Ring*, B. The concentric shaded bands on this ring have been on two or three of the most favourable occasions very well brought out. On this appearance I find the following notes in my journal:—

"1854, Nov. 26. The ring B is decidedly in *stripes*, and they are not *regularly* darker from the exterior one inwards. About one-fifth of the breadth of the ring, from its exterior edge, is very bright; then a narrow stripe is lightly shaded; immediately within that is a stripe decidedly lighter, though not so bright as the exterior fifth; next to that is a considerably darker stripe, and then a *much* darker one extending nearly to the interior edge,

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where there is a *very* narrow *bright* line, far less decided than it was in 1851 and 1852."

"Dec. 7. By brief views the step-like character of the shading on ring B is visible; and I think the *outer* shaded band is *darker than the next interior one*, as I noticed one night before."

"1855, Jan. 10. The bands of shading towards the interior edge of this ring are occasionally well brought out; and I think the *second* from the outside is not quite so dark as the first,—at least in some parts of it, for I doubt if it be quite uniform. The narrow bright line at the interior edge is visible, but is not, I think, so bright as it was the two previous apparitions."

3. *The obscure semi-transparent Ring*, C, has been very well seen on several occasions; and I have noticed nothing remarkable about it except the occasional variations of its tint in different parts. Respecting this I have recorded as follows:—

"1854, Sept. 26. The dark ring is plainly seen, and appears to-night of the same tint at both ansæ. Its semi-transparency is very obvious across the ball, the edges of which can at times be distinctly traced down to the inner edge of ring B."

"Dec. 26. The dark ring is remarkably clear: the following end is *ruddier* than the preceding."

"1855, Jan. 10. The ring C is wonderfully well seen in general: rather ruddy on the preceding side, slate-coloured on the following side. The ball is seen plainly, though faintly, through it."

4. *The Ball.* Of its appearances I have the following notes:—

"1854, Sept. 26. The belts on the ball are not very distinct. The southern boundary of the broad dark belt, which is immediately south of the equator, is *not uniform*, or parallel to its northern edge. The belt, therefore, varies in breadth in different parts, and is at present ($13^h\ 45^m$ G. M. T.) broadest near the eastern edge of the ball. There is a very narrow light line seen interruptedly crossing the belt from east to west, a little south of its middle. The rest of the southern hemisphere is nearly uniform in colour, except that round the south pole is a belt of rather darker tint, and at about 40° of south latitude there is a very narrow belt less dark than the polar one."

"Dec. 7. The annexed sketch" (in the journal) "shows the form of the shadow of the ball on the ring B. It does not extend to the ring A at all; but I think a very small portion ($0''{\cdot}2 \pm$) of the southern edge of the ball is projected upon A."

"Dec. 16, $12^h\ 30^m \pm$ G. M. T. The south pole, or rather the most southerly part of the ball, is very dark,—much darker than the ring A, and I think rather darker than the broad belt near the equator. This renders the contrast with the small visible portions of its shadow less evident. I feel pretty sure that the southern edge of the ball encroaches a trifle on the ring A. There are no distinct and well-defined belts on the ball now."

"1855, Jan. 10, $9^h \pm$ G. M. T. The whole of the southern hemisphere of the ball is ruddy, and the parts near the equator

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and at the southern edge are the darkest. Examined very carefully, and with all the various powers" (extending from 355 to 1000), "the position of the southern edge with respect to the edges of the rings at that part. The edge of the planet is so dark that it gives the impression sometimes of having a dark line there marking its contour. This darkness of the shading, at the very edge of the ball, renders it difficult to distinguish it from the division between the rings. But after long and careful examination, I am satisfied that *the ball extends over the division, and encroaches* 0″·2 *or* 0″·3 *on the ring* A. By carrying my eye across from the black division on one side to the other, I can see that, if continued in an uninterrupted line, it would *cut off a thin slice from the edge of the ball.* With very high powers (705 to 1000) the difference of colour of the southern edge of the ball, and the ring A at that point is more marked than with the lower powers; and long scrutiny with them confirms my impression that the ball encroaches slightly on A."

"$10^h\ 36^m \pm$ G. M. T. Applied an excellent Huygenian eyepiece, giving power 860. It is admirable. The difference of colour of the southern edge of the ball and the ring A is obvious; and there is no doubt at all of the slight encroachment of the ball on its interior edge. Finding the light of the planet produce a very unfavourable effect upon my eye while endeavouring to estimate the degree of encroachment of the ball on A, it occurred to me to apply my solar-eye-piece for the purpose of excluding the rest of the ball and rings, and leaving visible only the southern portion of the ball and the adjacent portion of the rings A and B. Power 506 (the highest, a double-convex lens). The effect is admirable. My eye having rested upon it for some time, the outline of the southern edge of the ball was far more distinctly seen than before, and leaves no doubt of its encroaching on the interior edge of A, to about 0″·3 by careful estimation. At times a little mottling can be discerned very near the southern limb of the ball. Its colour is very different from that of ring A; and it completely interrupts the black division which comes sharply up to the ball on both sides of it."

5. *The Shadow of the Ball on Ring* B. On this appearance I have noted as follows:—

"1854, Sept. 26. The shadow of the ball on ring B is nearly a straight line."

On Sept. 29 the projecting portion of the shadow, which has been noticed the last two or three years, was seen for the first time this season on the eastern side of the ball; cutting off the acute point of the ring B intercepted between the edge of the ball and the black division, as at *a* in the sketch, in which the appearance is much exaggerated.

At the same date I have remarked,—"I doubt if the shadow

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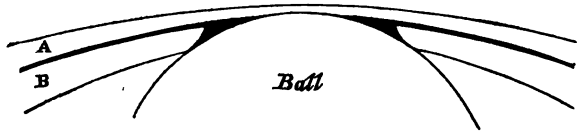
of the ball on ring B is really a straight line, though nearly so. It seems to be a little curved towards the southern end of it, close to the division." In the place indicated the edge is *convex* towards the ball.

"Nov. 26. Only a *very* narrow line of shadow from the ball falls on the west side, but there is a curious angular projection in the shadow on both the west and east sides of the apex of the ball."

"Dec. 7. The annexed sketch shows the form of the shadow



of the ball upon ring B." (Exaggerated in the sketch as respects the *size* of the shadow.)

6. *The Satellites.* I have usually estimated *Tethys* to be brighter than *Dione*, even when it has been nearer to the planet. This was remarkably the case on the 10th of this month, at $7^h \pm$ G.M.T. when both the satellites were near their greatest western elongation. At $11^h\ 19^m$ G.M.T., *Dione*, *Tethys*, and *Enceladus*, formed an equilateral triangle south-preceding the western end of the ring, thus,—

In No. 929 of the *Astron. Nachrichten*, is a most interesting account by Professor Secchi of the appearance of *Saturn* in the Munich equatoreal refractor, recently erected at the Observatory at Rome. The dimensions of the telescope are the same as those of the Dorpat refractor, the aperture of the object-glass being 9 Paris inches. The Professor characterises the night of Nov. 19 as one of extraordinary excellence, and doing full justice to the telescope. He describes the dark line on ring A as being just like a *pencil line* drawn upon it, which perfectly agrees with the views I have had of it since 1851; and with my description as "narrow, very dark, but not black." And when the dusky hue of the ring A is considered, it seems probable that this line would appear almost black if contrasted with a much brighter ground, such, for instance, as the exterior edge of the ring B. It deserves to be remarked that a dark line, precisely similar to this in appearance and situation, was seen on the *northern* surface of this ring, in the year 1838, by Professor Encke; and by Mr. Lassell and myself in 1842 (when we were not aware of Encke's observation). It may not be a *division* in the ring, as it was then supposed to be; but, if it is not, it is certainly extraordinary that precisely the same appearance should exist *on both surfaces of the ring*, and

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should be, as it would seem, a permanent phenomenon in respect of its situation on the ring, and the darkness of its shade.

Professor Secchi has also described the step-like concentric bands of shading on ring B, exactly as they were described by myself on October 26, 1851; and as I have occasionally seen them almost precisely in the same way to the present time, it may fairly be concluded that they form a permanent feature of this ring. The Professor does not notice the *comparatively bright line* at the interior edge of B, which seems to me to render that edge pretty definite, though it is certainly less bright now than it was two or three years ago.

In one important point the impression received by Professor Secchi differs decidedly from my own, as stated in the present paper: viz. the place to which the southern edge of the ball is seen to extend on the rings. He states that the opening of the ring is such, that *the upper edge of the ball exactly touches the interior edge of the black division between* A *and* B, which was visible throughout the whole of its elliptic perimeter. It is singular that, on the 26th of September I arrived at precisely the same conclusion; but the state of the air was not such as to permit the advantageous use of high powers; and my subsequent observations, under much better circumstances, and especially on the 10th of this month, convinced me that my first impression was erroneous, or that a change to a considerable, and in fact *unaccountable*, amount had taken place.

The *first* satellite of *Saturn* (now usually called *Mimas*) is stated by Professor Secchi to have been seen on November 19th, near its greatest western elongation; having been found by putting the planet nearly out of the field, and afterwards seen steadily with the planet in full view. It is surprising that he does not mention *Enceladus*, which must have been close to *Mimas* at that time, if the latter occupied the place indicated. As my telescope has not shown me *Mimas*, I cannot say where that satellite might have been; but my own observations prove that *Enceladus* occupied precisely the situation which the Professor has ascribed to *Mimas;* and I cannot but think it probable that further observations may have convinced him that it was not the *first*, but the *second*, satellite which he saw.

The *bright zone on the ball*, which commences almost precisely at the equator, and extends northwards as far as the ring permits it to be seen, forms one of the most conspicuous features of the planet. It has been repeatedly referred to by Professor Secchi, as *caused by the reflection of the sun's light from the surface of the ring*. Two considerations seem to me to be quite conclusive against its arising at all from that cause. One is, that this bright zone occupied precisely the same situation, and was very conspicuous, *when the plane of the ring passed through the sun.* (See "Remarks on the Planet Saturn," by the Astronomer Royal, in the *Greenwich Observations* for 1848, p. 44.) The other is, that the reflection of the sun's light from the *southern* surface of

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the ring, which now receives it, must necessarily fall upon the *southern* hemisphere of the ball, which has been remarkably dark ever since the southern surface of the ring has been illuminated; while the bright zone lies wholly in the *northern* hemisphere.—The remarkable obscurity of the southern hemisphere at the present time seems to indicate that the effect of the reflection from the surface of the ring is quite inappreciable as seen from the earth.

Wateringbury, Jan. 11, 1855,

Postscript.

"Jan. 14. The night proving fine, I again carefully examined *Saturn*, and made the following entry in my journal:—

"'$12^h\ 45^m$ G.M.T. *Saturn* is very fine at times, though about $3\frac{1}{4}^h$ past the meridian. It bears 705 very well; and with this power I have no doubt of the southern edge of the ball extending over the division between A and B, and encroaching a trifle on the interior edge of A. With low powers (355 or less) there is sometimes an appearance of the division extending across; but I am persuaded that this arises from the combined effect of the division coming up on each side so near the apex, and the *very* deep tint of the apex itself, which I think is *darker* than the darkest part of the broad belt close to the equator of the planet. It is certainly much darker than the ring A.'"

On Rating Chronometers by Lunars. By H. Toynbee, Esq., Commander of the Gloriana, East Indiaman.

Previous communications from Captain Toynbee on this subject will be found in the *Monthly Notices*, vol. ix. No. 7, and vol. xiv. pp. 19 and 243. In the present paper he shows that lunar distances taken from the stars and planets may be still more advantageously employed for determining chronometer errors and rates than those taken from the sun.

His method is to take several sets of good lunars on each side of the moon, if it may be, on consecutive days, and from the *mean* of the whole to deduce a chronometer *error* corresponding to the *mean epoch*. This is to be done every month. The *rate* is determined by a comparison of the *error* of each month with the error of the preceding month, and the examples adduced by Captain Toynbee of his own practice, and of that of Captain Quihampton of the Tudor, are exceedingly satisfactory.

It is to be feared that, partly from the practical difficulty attending lunar observations, and partly from the extended use of chronometers in navigation, this excellent and independent method of finding the longitude is going out of use. Yet in long voyages it must always be dangerous to rely solely on chronometers, unless they are numerous, and have been rated *with reference*

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to the temperature, which is not the practice of private raters. Even then, there are so many causes which may operate to change the rates of chronometers at sea, that a conscientious officer ought never to risk his ship and the lives of his crew upon the going of his chronometers, without the additional check and security which he derives from lunars.

In his last voyages, Captain Toynbee has relied chiefly on lunar distances taken from the stars and planets, and they are generally taken on each side of the moon the same night. When distances are taken from both sides of the moon, most of the instrumental errors affect the results contrary ways, and almost to the same amount. Thus the index error and want of parallelism in the dark glasses disappear from the mean of east and west distances; and if the arc is nearly the same in both cases, even the defective plane of the index-glass, the want of perpendicularity in the index and horizon-glasses, and the error of the arc of the sextant, are nearly eliminated, or at any rate greatly attenuated. The sextant is so difficult an instrument to make and to adjust correctly, that only the very best artists can be trusted; and, after all, the observer should use it in such a way as to reduce as much as possible the effect of unavoidable and unknown errors: for this purpose he can scarcely follow a better example than Captain Toynbee.

The errors of the lunar tables are sometimes larger than would seem likely from Captain Toynbee's instances, yet his method of extending the observations (from which his mean distance is included) over so large an arc will tend to diminish even this effect, which cannot be completely guarded against. The *number* of observations gets pretty well rid of *casual* error, either of observation or of computation; and the instrumental errors are compensated by making them tell contrary ways.

The following instances will suffice to show the system pursued by Captain Toynbee and its results.

By a mean of 10 distances on each side of the moon on March 6, 7, 8, 9, 10, 11, chron. Dent, 1759, was found,—

1854, March 9, $14^m\ 39^s{\cdot}6$ fast on G.M.T.

By 18 distances taken similarly between May 1 and May 10, the chron. was found,—

1854, May 6, $22^m\ 38^s{\cdot}2$ fast on G.M.T.

By 14 similar distances taken between August 29 and Sept. 6, the chron. was found,—

1854, Sept. 2, $40^m\ 35^s{\cdot}6$ fast on G.M.T.

By 8 similar distances taken between Sept. 27 and Oct. 6, the chron. was found,—

1854, Oct. 1, $44^m\ 25^s{\cdot}9$ fast on G.M.T.

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By 12 similar distances between Oct. 31 and Nov. 3, the chron. was found,—

1854, Nov. 1, $48^m\ 9^s{\cdot}9$ fast on G.M.T.

By 8 similar distances between Nov. 30 and Dec. 1, the chron. was found,—

1854, Dec. 1, $52^m\ 19^s{\cdot}8$ fast on G.M.T.

The partial results are good, but not extraordinary. The excellence of the *means* arises from the number of observations and the compensation of the opposite errors.

The chronometer seems to have performed with great regularity, as the rates from the above data are,—$8^s{\cdot}4$; $9^s{\cdot}1$; $7^s{\cdot}9$; $7^s{\cdot}2$; $8^s{\cdot}1$: all gaining.

The computed error of the chronometer from the above *errors* and *rates*, deduced solely from lunar distances, was verified at Calcutta and the Cape by the time-ball, and on the return to England by the maker; the differences amounted only to a few seconds.

The observations from which these results are deduced were taken while the moon was between new and full, and generally between 6^h and 10^m P.M.,—a time which best suited Captain Toynbee's convenience. He proposes hereafter to try morning observations, by which an independent error and new rate will be determined every fortnight. Whether this will repay the trouble depends on the chronometers, which, if good, and more than one in number, should keep the Greenwich M.T. for a month pretty safely. Towards the end of a voyage, the error of the chronometer cannot be too carefully determined, and once a fortnight is not too often.

Captain Quihampton says that he should have been a degree out of his reckoning if he had relied on his shore-errors and rates; but by his lunar rates he made the Island of Flores and the Islands of Scilly within a very few miles.

On the Orbit of α Centauri. By Eyre B. Powell, Esq.

In this paper the author gives the details of an investigation of the orbit of *α Centauri*. Two sets of elements are deduced by him. The following is the second set, which he appears to have derived from equations of condition involving the corrections of the original elements:—

τ	=	1858·012	
		° ′	
ϖ	=	29 33	
Ω	=	177 50	
γ	=	77 50	
e	=	·966	
n	=	4°·78	P = 75·3 years
	=	30′	

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The following is a comparison between the results of these elements and the corresponding results of observation, where P_c denotes the calculated and P_o the observed angle of position; and D_c the calculated and D_o the observed distance.

Date.	P_c	P_o	$P_c - P_o$	D_c	D_o	$D_c - D_o$	Observer.
	° ′	° ′	′	″	″	″	
1826·012	212 23	213 11	−48	20·66	22·45	−1·79	Dunlop
1830·012	214 46	215 2	−16	19·1	19·95	− ·85	Johnson
1834·79	218 15	218 30	−15	16·84	17·4	− ·56	Herschel
1837·34	220 32	220 42	−10	15·48	16·11	− ·63	Herschel
1846·866	235 33	234 18	+75	9·85	9·82	+ ·03	Jacob
1848·023	238 53	237 59	+54	8·61	8·05	+ ·56	Jacob
1850·956	251 13	250 43	+30	6·5	5·97	+ ·53	Jacob
1853·049	266 15	267 34	−79	5·03	4·55	+ ·48	Jacob
1854·003	276 19	276 20	− 1	4·45	4·21	+ ·24	Jacob & Self
1854·632	284 19	283 32	+47	...	...	...	Self

The author remarks that the time of the next periastral passage probably lies between 1857·5 and 1858·5, and that the semi-axis major of the orbit is a little greater than 30″.

The following is a letter from Mr. Hind to Admiral Smyth in reference to the orbit of the same star:—

"I believe a paper upon the elements of *α Centauri* was referred to you for report yesterday. I have recently deduced an orbit for the same star, not being aware that any one else was engaged upon the same investigation. My results, in which I have some confidence, are founded upon all the micrometrical measures from 1834 onwards, including the excellent series by Capt. Jacob down to the present year. I have thought you might like to have the elements for comparison with those given in the communication to the R.A.S. which I have not seen:—

Per. pass.	1859·42
☊	16° 42′
λ	26 2
Eccentricity	0·7752
Mean annual motion	+ 4°·448
γ	62° 53′
α	13″·57
Period	80·94 yrs.

"These elements agree very well with the observations, so far as I have examined them.

"*Dec.* 9, 1851."

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On an Appearance seen in the Moon. By Robert Hart, Esq.

(*Letter to the Astronomer Royal.*)

"On the night of the 27th December, 1854, between 6 and 7 P.M., the moon was very bright. I had brought my 10-inch reflector to bear upon the moon; on the shaded side of the disk I observed a white spot, where I have marked it on the sketch. As it was of the colour of the light of the moon, and not like star-light, I thought it part of the moon; but as it disappeared in less than a minute after I first observed it, I concluded it was a star eclipsed by the moon.* I now turned my attention to the light part of the disk, and my eye was at once attracted by an appearance I had never seen before on the surface of the moon, although I have observed her often during these last forty years. She was $8^d\ 4^h$ old at the time, and just on the edge of the light, where I have marked on the sketch, there were *two luminous spots*, one on either side of a small ridge, *which ridge was in the light*, and of the same colour as the moon; but these spots were of *a yellow flame colour*, while all the rest of the enlightened part was of a snowy white, and the mountain-tops that were coming into the light, *and just on the shadow side of these spots, were of the same colour as the moon.* The lights of these spots were *like the light of the setting sun reflected from a window a mile or two off.* I observed it for five hours. I thought them rather less bright than as first seen, *but very little less;* so bright were they, when the instrument was the least thing out of focus, they showed *rays around them as a star would do.*

"As I live about two miles out of Glasgow, I had no scientific friend with me at the time, but I called the attention of three gentlemen, my neighbours, and my own household, and they all described the appearance as I saw it myself, and have given above.

"I would have followed it longer had I been able, but the wind was very cold, 20°·5, and I had no shelter, as I take my telescope outside. I left off about 12 P.M.; and from the cloudy weather, I never saw the moon until she was nearly full, then only a few minutes, and again on the 10th January, at 4 A.M.; but the haze was too thick. I thought I might see them in the shade, but I could not: I have never seen her since.

"May I ask the favour of your laying this before the Astronomical Society, that we might learn if any of the members have observed this appearance in the moon, or have heard that it was seen by others. I shall be happy to learn if it has been taken notice of by any other observer, and their opinion of it.

"It appeared to me, from the brightness of the light and the contrast of colour, *to be two active volcanoes or two mouths of one in action.*

* This was, no doubt, the occultation of μ *Piscium*, which, according to the *Nautical Almanac*, was in conjunction with the moon at $6^h\ 54^m\ 10^s$, on the evening of December 27, 1854.—Editor.

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"If there was no star eclipsed by the moon at or about 7 P.M. that night, the white spot that was more like one of the mountain-tops tipped with light than a star, may have had some connexion with it.

"*Cesmook Park, January 15th*, 1855."

Discovery of a New Comet (Comet I. 1855).
By M. Winnecke.

On the morning of the 15th of January, M. Winnecke discovered at Berlin, in the vicinity of γ *Hydræ*, a faint granulous nebula, the estimated position of which was,—

Jan. 14, 18^h	$225° 20'$	$-27° 11'$

On the next morning it was found to have shifted its place. The following observation of its position was obtained by the discoverer and M. Bruhns:—

	Berlin M.T.	R.A.	Decl.
	h m s	° ′ ″	° ′ ″
Jan. 15	18 4 16·2	226 5 15·4	−27 15 5·3

Apparent place of the star of comparison,—

	R.A.	Decl.
	° ′ ″	° ′ ″
Argel Z 373, No. 57	226 38 16	−27 18 42·4

The daily motion is consequently,—

In R.A. about	$+45'$
Dec. —	$-4'$

The comet has also been discovered independently by M. Dien, at the Imperial Observatory, Paris.

M. Bruhns has calculated an ephemeris of the comet, extending from Jan. 14 to Feb. 17, printed copies of which have been circulated by M. Peters, the editor of the *Astronomische Nachrichten*.

The weather has hitherto been very unfavourable for observing this comet in England.

On the Orbit of the Binary Star σ Coronæ Borealis.
By Eyre B. Powell, Esq.

"Three orbits have been already arrived at for this star, viz. that computed by Sir John Herschel, and the approximately inter-accordant ones of Messrs. Hind and Mädler; it may, therefore, appear, at first sight, a work of supererogation to investigate a fresh set of elements. The startling difference, however, that exists between Sir J. Herschel's orbit and the other two, will,

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perhaps, be held to excuse my troubling the Society with the present communication.

"By the graphical method I obtained the following results for the apparent orbit of σ.

$a = 2''{\cdot}725$
$e = {\cdot}6479$
Minimum distance = $1''{\cdot}27$
Position for do. = $91^\circ\ 20'$
Maximum distance = $2''{\cdot}74$
Position for do. = $324^\circ\ 30'$

"The apparent ellipse then gave the elements of the real orbit as follow:—

σ = 1829·7
ϖ = $102^\circ\ 50'$
Ω = 3 8
γ = 45 6; $\lambda = 96^\circ\ 53'$
e = ·3887
n = + 1°·5, Period = 240 years.
d = 2''·94

"I annex two tables, one affording a comparison between the results of computation and those of observation for angles of position, the other effecting the same object for distances.

Comparison of Calculated and Observed Positions.

Date.	θ_c	θ_o	$\theta_c - \theta_o$	Observers.
1780·6	347 48	347 32	+ 16	H sec. *Cycle* *
1781·79	348 57	347 32	+1 25	H sec. H
1802·74	13 58	11 24	+2 34	H
1822·48	66 44	66 24	+ 20	H & S, Σ†
1824·45	75 57	75 10	+ 47	H & S, S‡
1826·77	87 37	89 0	−1 23	Σ
1830·52	107 3	106 51	+ 12	H, D, Sm§
1831·34	111 9	111 32	− 23	D
1832·459	116 37	115 26	+1 11	D, Sm‖
1833·47	121 20	120 39	+ 41	D, Sm¶
1835·5	130 11	130 54	− 43	Sm
1839·67	145 27	145 6	+ 21	Sm
1843·35	156 8	155 54	+ 14	Sm
1846·22	163 6	163 45	− 39	J
1853·513	177 7	176 59	+ 8	J & Self**

* I believe the *Cycle* follows the *Philosoph. Trans.* See vol. v. Astron. Soc. *Memoirs*.

† 1821·3 65° 15′ H & S; 1822·67 61° 0′ Σ; 1823·47 72° 56′ H & S.

‡ 1823·47 72° 56′ H & S; 1825·44 77° 31′ S.

§ 1830·28 105° 5′ H; 1830·52 107° 17′ D; 1830·76 107° 36′ Sm.

‖ 1832·549 115° 57′ D; 1832·37 114° 54′ Sm.

¶ 1833·36 120° 37′ D; 1833·58 120° 42′ Sm.

** 1853·142 177° 54′ J; 1853·348 175° 12′ Self; 1854·048 177° 52′ J.

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Comparison of Calculated and Observed Distances.

Date.	d_c	d_o	$d_c - d_o$	Observers.	Remarks.
	″	″	″		
1823·47	1·34	1·45	−·11	H & S	
1826·77	1·28	1·3	−·02	Σ	
1830·52	1·29	1·26	+·03	H, Sm	1830·28, 1″·22 H; 1830·76, 1″·3 Sm
1832·37	1·33	1·4	−·07	Sm	
1833·47	1·36	1·25	+·11	D, Sm	1833·36, 1″·3 D; 1833·58, 1″·2 Sm
1835·5	1·43	1·4	+·03	Sm	
1839·67	1·62	1·6	+·02	Sm	
1843·35	1·7975	1·8	−·0025	Sm	
1846·22	1·94	1·87	+·07	J	
1853·7	2·25	2·21	+·04	J	

♅ stands for Sir W. Herschel; H for Sir J. Herschel; Σ for Prof. Struve; S for Sir J. South; Sm for Admiral Smyth; D for the Rev. Mr. Dawes; J for Captain Jacob.

"It is not necessary to dwell upon the foregoing; but I would remark that my orbit approaches much nearer to the early one of Sir J. Herschel than it does to those computed by Messrs. Mädler and Hind at more recent dates. So far as I am able to judge, there does not appear to be ground for believing the period to extend over six or seven hundred years.

"I did not think it worth while to form equations of condition with the view of correcting the elements, as the orbit represents the observations, more especially those of distance, with very considerable accuracy: indeed in no other case have I arrived at equally respectable results, I will not say after the first, but even after the third or fourth approximation.

"*Madras, Nov. 20th, 1854.*"

Observations of the Zodiacal Light in 1854.

By T. W. Burr, Esq.

In continuation of my previous notice of the zodiacal light, I have to report that in the spring of 1854 I observed it on the evenings of February 16th, 18th, 23d, 25th, and 26th; March 17th, 21st, 23d, 27th, and 30th; and April 17th. The first and two last appearances were mere glimpses, but on February 18th the view was exceedingly good; and those of 21st March from 8^h 10^m to 9^h Greenwich mean time, and 23d March from 8^h to 8^h 30^m, were as fine as possible in this latitude and locality.

It is unnecessary to describe its course and appearance with any great minuteness, as there is always a difficulty in ascertaining its exact extent. Generally it very much resembled the description I gave of it in the preceding year, extending in the early

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observations to the vicinity of the Pleiades, and in the later ones to that of *Aldebaran*. Upon the whole its boundaries were not so well defined as in 1853.

I have observed occasional sudden variations in its brightness, but have been unable to detect anything like a periodicity in these changes; and knowing the great influence of atmospheric causes on objects having low altitude, I should hesitate to ascribe them to a real variation in the brightness of the light. Generally speaking, when best seen, the brilliancy was greater than that of the milky way, which was usually visible at the same time in the north-west.

Highbury, January 1855.

On the Pendulum Experiment for Illustrating the Rotation of the Earth. By Lieut. Ashe, R.N.

After some geometrical illustrations, the author gives the following description of the pendulum employed by him in these experiments:—

"In the centre of an equilateral plate a small hole was drilled, through which a piece of the best pianoforte wire, a foot in length, was put, fitting tight, and secured to the plate by the wire having a nut screwed on the end of it, and a loop was turned at the other end of the wire. To this steel circular spring a copper wire, stretched and rubbed, was attached; the length of pendulum was 52 feet, and the weight, a leaden sphere, 17lbs. The plate was firmly screwed to a beam in the steeple of Chalmers' Church of this city (Quebec); the pendulum was protected from any current of air. At about the height of the eye, when sitting down, a wooden circle, 4 inches broad, 3 inches thick, and 8 feet in diameter, was fixed and graduated on the inner edge, so that by placing the eye on a level with the circumference, the time of the wire coinciding with each degree could be noted with great nicety.

"I found steel wire more unsatisfactory in its results than any other sort, in consequence of its being so very sonorous,—so much so, that the noise of a cricket, or any other sound that would harmonise with the wire, produced undulations."

Description of Shepherd's Galvano-Magnetic Regulator. By Henry S. Ellis.

(*Communicated by Dr. Lee.*)

I beg to forward a description of the galvano-magnetic regulator and apparatus, which have been manufactured by the

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patentee, Mr. C. Shepherd, of Leadenhall Street, and placed in the Exeter Guildhall, for the purpose of giving hour-signals to the cathedral clock, and denoting uniform time throughout this city. In further explanation of the same I enclose three drawings.*

No. 1 is a general outline of the regulator, and is very similar to the one at the Greenwich Observatory, except that there is no contact-spring in connexion with the hour-wheel, which was found to be unnecessary.

No. 2 is the dial of the regulator, and shows the contact-springs for giving hour-signals to the cathedral clock. The lower pair of springs (A) is brought into contact with each other every hour, when the pin (B) in the minute-wheel presses them together; and similarly the upper pair of springs (C) are brought into contact every minute by the pin (D) in the stud underneath the seconds hand; but the whole circuit, between the batteries and the magnets at the cathedral, is not completed until both the lower and the upper pairs of springs make contact simultaneously; and as seven seconds elapse between the signal and the first blow struck on the bell, the contacts are made in anticipation of the hour, so that the striking shall coincide exactly with time shown by the regulator.

No. 3 is the old striking part of the cathedral clock, and the new galvano-magnetic apparatus which is placed in electric connexion with the regulator. When the hour-signal is given, the armature (A) is drawn down on the poles of the magnets (B B), the perpendicular to it is brought forward, and the hammer (D) falls on the tail of detent (E); the stop-piece (F F) is then released, and the striking train performs its work. The pin-wheel (G) raises the hammer (D) again into its horizontal position in readiness for the next hour.

The whole of the iron work in the new apparatus was made much heavier than is absolutely necessary. At first twenty-four of Smee's batteries were requisite to draw the armatures to the magnets; but by lessening the weight of the hammer (C) and the counterpoise (H), it has been found that nine of the same batteries are sufficient—although, to guard against a want of power, and to prevent the necessity of frequent charging, a larger number is now applied. Other parts might be made considerably lighter, and were that done probably about six batteries would be sufficient.

It may be remarked, that if a simple galvanometer could be attached to show the waste of battery power it would be a great convenience.

It is proposed to place a large electric dial, or dials, on the exterior of the Guildhall, and some of the distant parishes have it in contemplation to purchase new clocks, and to put them in connexion with the Guildhall regulator. Indeed, I believe it would

* See Illustrations at the end of this Notice.

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be practicable to connect all the existing old clocks and dials with it, at a trifling cost beyond that of laying down the electric wires.

When the Electric Telegraph Company bring their wires into the city, it is hoped that they will give the Exonians the benefit of one of the Astronomer Royal's daily signals—the deflection of a needle in immediate proximity to the regulator would be sufficient. For the present a 12-inch sympathetic dial of Shepherd's in my own observatory reports to me the performance of the regulator; and there will be little difficulty in making all the public clocks indicate and strike Greenwich mean time, within a second, throughout the year.

In conclusion, I have to acknowledge, with my very best thanks, the readiness with which the Dean and Chapter permitted the cathedral clock to be connected with the Guildhall regulator; and also the cordial support received from the Right Worshipful the Mayor, the Town Council, and others interested in the carrying out of these chronometrical arrangements.

Exeter, November 8, 1854.

Observations of the Solar Spots. By Dr. Wolf.

1854.	Days of Observation.	Days free of Spots.	Number of new Groups in the Month.
January	23	4	4
February	22	6	4
March	29	6	7
April	25	2	6
May	28	7	7
June	30	1	9
July	29	4	7
August	30	4	4
September	30	5	7
October	24	8	5
November	16	0	4
December	18	4	4
	304	51	68

The following table exhibits a synopsis of the condition of the sun during the last six years, in regard to the relative mean number of groups of spots visible on his disk in each month of the year. In order to take some account of the magnitude of the groups, the results are increased by one-tenth of the number of spots visible.

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	1849.	1850.	1851.	1852.	1853.	1854.
Jan.	17·3	10·3	9·0	7·6	4·2	1·4
Feb.	14·3	10·0	10·0	6·3	5·1	1·7
March	10·1	8·8	6·8	6·7	3·3	1·9
April	11·5	3·6	6·1	6·7	5·0	3·1
May	9·6	5·4	7·0	5·4	3·5	2·2
June	9·8	10·5	6·5	4·8	5·0	2·0
July	9·0	4·8	3·1	4·4	4·7	1·9
Aug.	7·0	6·7	6·2	4·4	5·3	1·6
Sept.	10·1	9·1	7·4	3·6	3·1	2·4
Oct.	9·0	9·2	6·3	10·2	4·5	1·7
Nov.	11·9	4·8	6·1	6·7	2·8	2·0
Dec.	10·9	5·2	6·8	5·3	2·6	1·5
Sum	130·7	88·4	81·3	72·1	49·1	23·4

These results seem to agree with the period of the solar spots which Dr. Wolf has assigned, according to which the minimum of the number of groups falls in the latter half of the year 1855.

Dr. Drew has for sale a 30-inch transit-circle by the late Mr. Jones, of Charing Cross. The telescope is of $3\frac{1}{2}$-feet focal length, with an object-glass of $3\frac{1}{4}$ inches diameter. The circle is read off by three microscopes mounted on solid stone arms; is provided with a micrometer eye-piece and every means for the nicest adjustment. The collimation-error may be corrected either by reflection or by the aid of two collimators which accompany the instrument. Price, with the stone mounting, 100*l.*

A drawing and description of the instrument will be forwarded by Mr. Williams, the Assistant-Secretary of the Society, to any person applying for the same.

ERRATA.

The reader is requested to correct the following errata, which were committed in the translation of Professor Hansen's paper on the Construction of New Lunar Tables, &c., inserted in the *Monthly Notices* for November last:—

Page 12, line 8 from bottom, *for* a large number, *read* a moderate number.

— 14, line 1, *for* were different, *read* were not different.

— 14, last line, *for* greater than unity, *read* less than unity (kleiner wie Eins).

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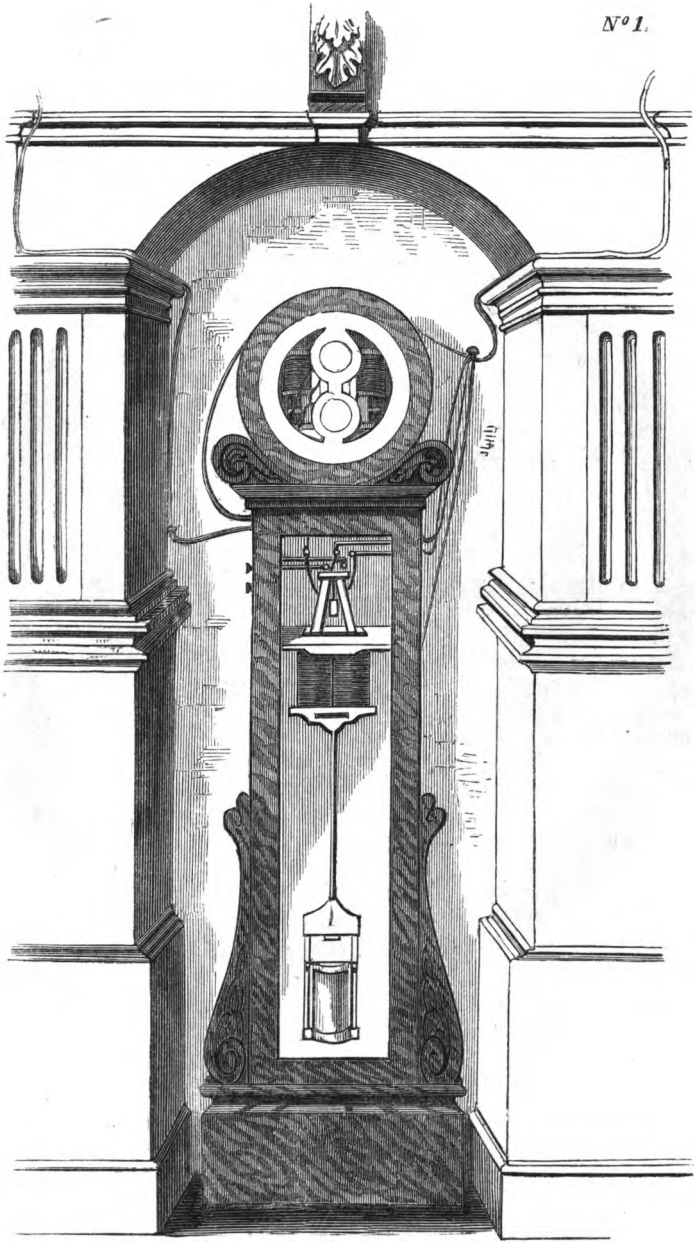

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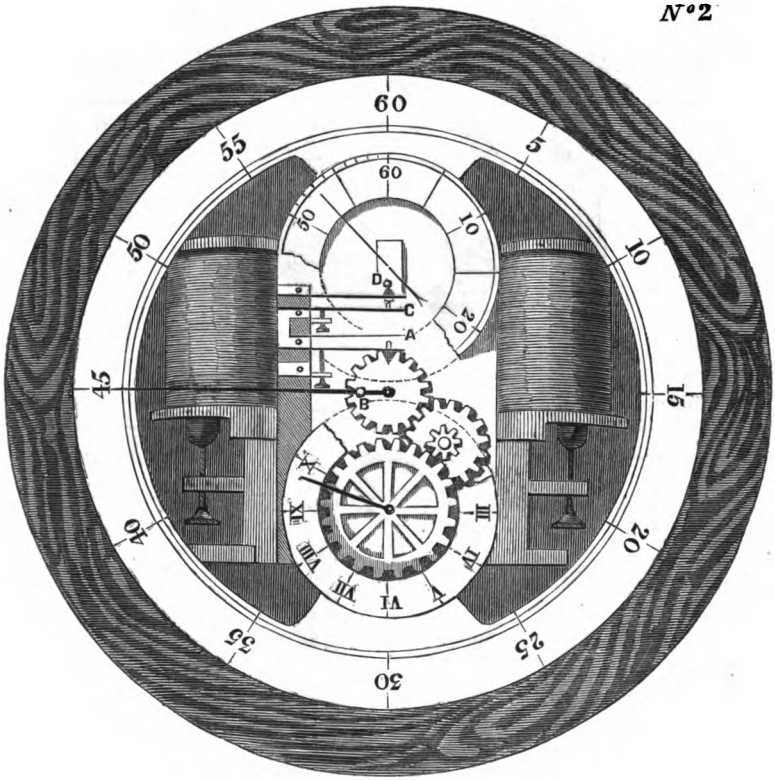

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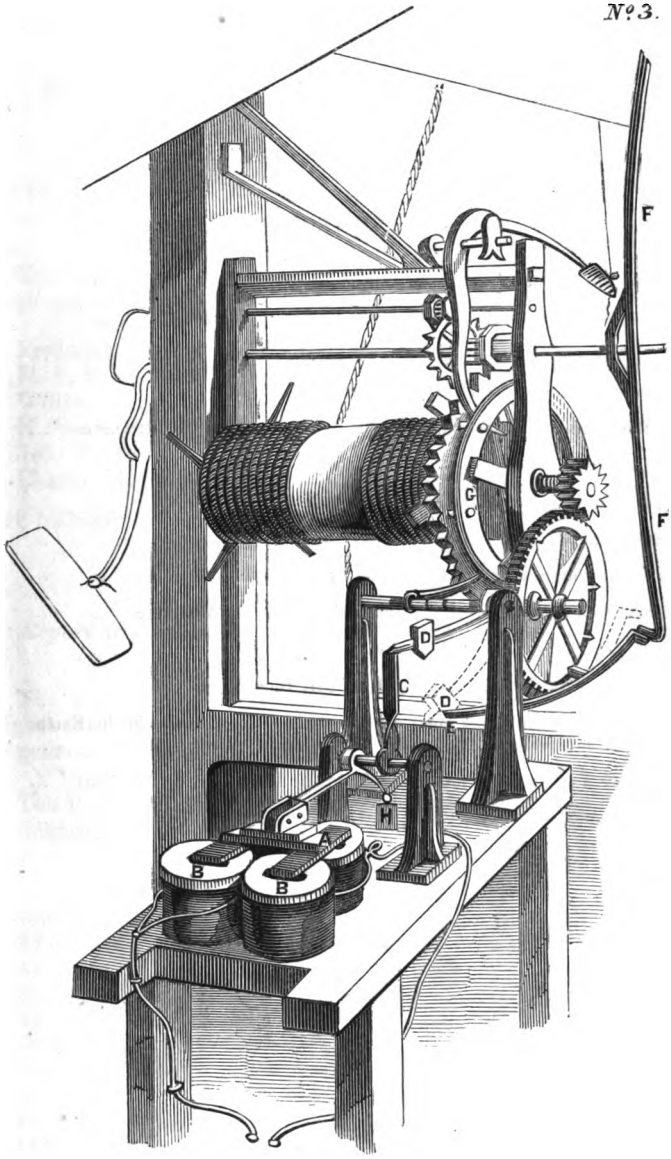

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *February* 9, 1855. No. 4.

THE Annual General Meeting of the Society, G. B. AIRY, Esq., President, in the Chair.

Frederick Brodie, Esq., The Gore, Eastbourne, Sussex;
H. S. Ellis, Esq., Exeter;
William Lethbridge, Esq., St. Paul's School;
H. W. Buxton, Esq., 37 Abbey Road, Regent's Park;
Jon. T. Owen, Esq., Swansea; and
Charles H. Wild, Esq., 103 St. Martin's Lane,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Thirty-fifth Annual General Meeting of the Society.

The Council, in presenting the following Report, desire to congratulate their constituents on the state of the Society, and on the general progress of the science of astronomy.

The Report of the Auditors, subjoined, will show the state of the finances:—

RECEIPTS.

	£	s.	d.
Balance of last year's account	458	3	11
By dividend on £2619 5*s.* 9*d.* 3¼ per Cents	41	6	6
By ditto on £1650 Consols	23	6	2
By ditto on £2832 16*s.* 11*d.* 3¼ per Cents	43	7	0
By ditto on £1650 Consols	23	6	2
On account of arrears of contributions	55	14	0
104 contributions (1854–55)	218	8	0
4 ditto (1855–56)	8	8	0
12 compositions	252	0	0
24 admission fees	50	8	0
15 first year's contributions	26	5	0
Sale of Publications	59	10	6
	£1260	3	3

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EXPENDITURE.

	£	s.	d.
Cash paid Mr. Basire, engraver	18	4	6
J. Rumfitt, bookbinder	9	0	4
Mr. R. Grant	40	0	0
George Barclay, printer	114	0	8
Mrs. Jones (Lee Fund)	3	5	6
Mr. R. Grant	10	10	0
Investing compositions of E. B. Powell, C. G. Prideaux, A. B. Martin, T. W. Burr, James Cockle, W. H. Besant, and W. Huggins, Esqs., and Capt. Toynbee	189	0	0
J. Rumfitt	8	15	0
Mr. R. Grant	10	0	0
Investing compositions of James Samuel and J. J. Burman, Esqs.	42	0	0
James Basire, engraver	22	6	0
Mr. R. Grant	10	0	0
George Barclay	208	17	6
J. Rumfitt	9	15	11
Taxes { 1 year's land tax 5 12 6; 1 year's property tax 1 9 2	7	1	8
J. Williams' salary	100	0	0
Ditto commission on collecting £418 14*s.* 6*d.*	20	18	6
Charges on books, and carriage of parcels	3	15	2
Postage of letters and Monthly Notices	36	15	9
Porter's and charwoman's work	24	14	4
Tea, sugar, biscuits, &c. for evening meetings	13	13	0
Coals, candles, &c.	15	6	6
Waiters attending meetings	3	17	0
Sundry disbursements by the Treasurer	23	14	3
Balance in the hands of the Treasurer	314	11	8
	£1260	3	3

Assets and present property of the Society:—

	£	s.	d.
Balance in the Treasurer's hands	314	11	8
1 contribution of 7 years' standing 14 14 0			
4 ——— of 6 ditto 50 8 0			
3 ——— of 5 ditto 31 10 0			
3 ——— of 4 ditto 25 4 0			
5 ——— of 3 ditto 31 10 0			
16 ——— of 2 ditto 67 4 0			
18 ——— of 1 ditto 37 16 0	258	6	0
Due for publications of the Society	12	12	6

£1650 3 per Cent Consols.

£2832 16*s.* 11*d.* 3¼ per Cent Annuities.

Unsold publications of the Society.

Various astronomical instruments, books, prints, &c.

The balance of the Turnor Fund (included in Treasurer's balance above) 24 8 6

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Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	39	VII.	221	XVII.	247
I. Part 2	82	VIII.	208	XVIII.	258
II. Part 1	100	IX.	214	XIX.	273
II. Part 2	62	X.	226	XX.	272
III. Part 1	129	XI.	236	XXI. Part 1 (separate).	333
III. Part 2	149	XII.	243	XXI. Part 2 (separate).	62
IV. Part 1	151	XIII.	262	XXI. (together).	210
IV. Part 2	164	XIV.	447		
V.	178	XV.	275	XXII.	251
VI.	198	XVI.	262	XXIII.	444

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1854	140	182	64	6	392	58	450
Since elected	11	13	...	...	24	4	...
Deceased	−2	−4	−3	...	−9	−2	...
Removals	2	−2	...	...	...	...	...
Resigned	...	−3	...	...	−3	...	...
February 1855	151	186	61	6	404	60	464

The instruments belonging to the Society are now distributed as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Owen* portable quadruple sextant,
The *Beaufoy* circle,
The *Beaufoy* clock,
The *Herschelian* 7-foot reflector,
The *Greig* universal instrument,
The *Smeaton* equatorial,
The *Cavendish* apparatus,
The *Lee* circle,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,

are in the apartments of the Society.

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The Brass quadrant, said to have been *Lacaille's*,

is in the apartments of the Royal Society.

The Standard scale

is in the charge of the Astronomer Royal, with the consent of the Council, to be employed in the construction of a new Standard Measure, under the direction of the Standard Committee.

The *Wollaston* telescope,

having been returned by the representatives of the late Professor Schumacher, is now in the hands of Mr. Dollond for repair.

The remaining instruments are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The other *Beaufoy* clock,
The two invariable pendulums, } to the Royal Society.
The Variation transit (late Mr. Shearman's), to Mr. Gravatt.

Among the presents will be found some valuable editions of different writings of Galileo, presented by Sir W. C. Trevelyan, Bart. In thus thanking the donor for his very acceptable contribution to our library, the Council remark that a doubt seems to have existed as to whether it would be worth while to offer the works to a library professedly astronomical, and which, therefore, might be presumed to possess copies. It should be known that, up to the present time, our library is by no means rich in anything but the astronomy of the present century. The great bulk of it, with the exception of the books of the Mathematical Society, consists of presents of the works of men who have lived in our own day. The application of the Turnor Fund will surely, though very slowly, remedy this defect: in the meantime, works of the astronomers of the three centuries preceding the present one will be valuable additions, and will be duly appreciated.

The medal has been awarded to the Rev. W. R. Dawes, for his astronomical labours generally, but especially for those recorded in our *Memoirs*. The President will, in the usual manner, lay before the Society the grounds of this award, and present the medal, at the conclusion of the ordinary business.

The twenty-third volume of the *Memoirs* has been published some time since. The Council would direct the attention of the Fellows of the Society to a Paper in it by Lord Wrottesley, containing a Catalogue of the right ascensions of somewhat more than one thousand stars. This is not the first occasion on which the Council have had the pleasure of directing attention to the labours of this excellent observer. From the recent researches of various astronomers on the motion of the solar system in space, and the

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remarkably accordant results which have been deduced from such inquiries, the determination of the places of the stars acquires a high degree of importance in a speculative point of view, independent of its otherwise inestimable value in forming a sure groundwork upon which to establish the fundamental points of astronomy.

The same volume of the *Memoirs* also contains some results of Mr. Lassell's observations at Malta, to which allusion was made in the last Report of the Council.

The *Monthly Notices* still continue to be conducted upon the plan introduced by Mr. Sheepshanks about two years since, and to which allusion was made in the last Report of the Council. It may now be stated pretty confidently that this modification of the original plan has answered the purpose for which it was designed, and appears to have given general satisfaction to the Fellows of the Society. Indeed, it could hardly be doubted, that by adapting the form of publication so as to co-operate with the *Astronomische Nachrichten*, instead of clashing with the arrangements of that valuable journal, the *Monthly Notices* would assume a more special character, and their utility in promoting the interests of astronomical science would thereby be materially enhanced. It may, perhaps, be unnecessary to repeat, that although it has been deemed expedient to assign to the foreign periodical just referred to the publication of all ephemerides and observations of the minor planets, as being the channel best adapted for such communications, still there remains an immense field of observation and research, in respect to which the pages of the *Monthly Notices* offer a readily accessible medium of intercourse among astronomers. If there is one point more than another which the Council would urge upon the Fellows of the Society in regard to promoting the usefulness of the *Monthly Notices*, it is the importance of subjecting to a searching process of computation the numerous and valuable results of observation which emanate annually from the public observatories of the country, and also occasionally from several private observatories. With respect to observations of an exceptional character, such as those relating to comets, double stars, and satellites, it is gratifying to find that an increasing desire has recently been evinced to apply the rich store of observations which have been accumulated in recent years towards improving some of the elements of astronomy. It cannot be expected that those more extensive series of observations constituting the normal results established in continuous succession at the public observatories of the country will so frequently form the subject of discussion. Still, the Council cannot refrain from reminding the Fellows of the Society that the facilities for such researches have been very much increased in recent years. From the circumstance of the results of the Greenwich observations being now published annually in a separate form, which may be obtained gratuitously upon application at the Royal Observatory, a confident hope is enter-

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tained that our countrymen will not be slow to avail themselves of those precious materials.

It will have been remarked by the Fellows of the Society, that since the commencement of the present session the *Monthly Notices* have appeared upon a stamped sheet. In regard to the easier transmission of the *Notices* to their various destinations, the practicability of employing a better quality of paper than hitherto, as well as several other obvious advantages, the expediency of this alteration cannot fail to suggest itself to every one. It is to be hoped, more especially, that it will have the effect of facilitating the transmission of the *Notices* to various countries on the Continent, where, hitherto, serious impediments have existed in regard to this object.

Our obituary list for the past year contains the names of Baron von Lindenau, Dr. Petersen, and M. Mauvais, Associates, and of the following Fellows:—Captain Blackwood; Edward Riddle, Esq.; Lieut. St. John; Professor Scott; Robert Snow, Esq.; the Rev. J. B. Wildig; and Dr. Whittaker. The official recognition of the loss of the Arctic expedition has made it the duty of the Council to add to the list of this year the names of Sir John Franklin and Captain Crozier.

Bernard von Lindenau was born in Altenburg, the capital of Saxe Altenburg, on the 11th June, 1780. His father was a wealthy proprietor. He studied law and political economy at Leipsic, but even there gave his leisure hours to his favourite pursuits, mathematics and astronomy.

On the completion of his legal studies, Von Lindenau repaired to the Observatory of Seeberg, where he continued his astronomical education under the direction of Von Zach. His progress was proportioned to his talents and unwearied industry, and when Von Zach left Seeberg in 1804, the direction of the Observatory and editorship of the celebrated *Monatliche Correspondenz*, which was established by Von Zach, were both committed to him.

In 1814, Von Lindenau left the Observatory in Nikolai's charge, while he made the campaign as adjutant to the Duke of Saxe Weimar. After the war, though he returned to the Observatory, and did not altogether neglect astronomy, his attention was directed to government and administration. The capacity with which he conducted the business intrusted to him, and the services which he rendered to Saxe Altenburg and Saxe Gotha, at that time united, raised him in a few years to the first offices in the State. He was prime minister of the two dukedoms, while he still retained, under the title of curator, the supervision of the affairs of the Observatory and of its working.

When the reigning line of Gotha and Altenburg failed, and those possessions were united to Saxe Coburg, Von Lindenau entered into the service of the King of Saxony, and was for some years the representative of Saxony at the Frankfort Diet. He

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then returned to Dresden as Minister of the Interior, an office which he actively discharged till 1843. In this year he took leave of public life, and spent the remainder of his days at Altenburg on his own property. Although his important ministerial duties forbade, during a large portion of his life, much personal contribution to astronomy, he never lost sight of this science. He took a lively interest in everything which occurred, and kept up as active a correspondence with his astronomical friends, Bessel, Gauss, Schumacher, Hansen, Olbers, Von Zach, Encke, &c., as his public occupations permitted.

On his retirement, Von Lindenau gave himself up to science and art, and resumed his original activity. He executed scientific researches, completed his rich library, and built a museum, which he furnished with the treasures he had already purchased of ancient and modern art.

Providence kindly favoured him with almost uninterrupted good health, till within a few weeks of his decease.

He died on the 21st May, 1854, at ten in the morning, possessing his intellects unclouded to the last.

After this sketch of Von Lindenau's scientific and public services, it should be added that he is to be classed among those men in whom great intellectual powers are united with the Christian virtues. A manly uprightness, which knew no way but that which is straightforward, guided him in all his dealings, and his sole desire was to act according to his convictions. With this he combined great benevolence. A large part of the income which he derived from several inheritances was devoted to charitable purposes, and he had the happiness to alleviate many a want and to dry many a tear. He bequeathed nearly half of his fortune to public or charitable purposes. His library and museum, with the means for supporting them, were left to Altenburg, 60,000 thalers to the charitable institutions of Altenburg, and 30,000 to those of Gotha, besides legacies to particular persons. By his will he bequeathed his chronometers to his scientific friends; the chronometer presented to him by Frederic VI. of Denmark to Professor Hansen, and two by Emery to Professors Encke and Möbius, with a request that they should descend in their families as a remembrance of him. It may truly be said of Bernard von Lindenau that his memory will live not only in the results of his industry and talent but in his Christian virtues.

Adolphus Cornelius Petersen was born July 23, 1804, at Vester-Bau, in Schleswig, where his father was a farmer. Up

* Professor Schumacher used to tell with great humour a story of Von Lindenau, which marks well his chivalrous character. When the Allies had entered Paris, one of his fellow-officers, congratulating him on their success, applied a term of insult to Napoleon. "We Germans may hate Napoleon," was Von Lindenau's reply, "but not insult him." The dispute grew so warm, that it ended in a duel, in which Von Lindenau received a wound, that afterwards caused him much trouble and a painful operation.

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to the time of his confirmation, in March 1820, he attended an elementary school at Buhrkarl, and occasionally assisted his father in husbandry. He subsequently learned land-surveying, and got so well acquainted with the subject in a year, that he was able to undertake the execution of plans without assistance. From 1820 to 1824 he was partly employed in measuring, and partly in plan-drawing. In the spring of 1825 he was engaged on the drainage of the district of Tondern, which had suffered greatly from the floods of the 3d and 4th of February. Here he got acquainted with the late Captain Von Caroc, one of the ablest officers employed in the measurement of the Danish arc of the meridian; and through him Petersen became known to Professor Schumacher, who invited him to fill the place of Thomas Clausen, as assistant-observer at the Altona Observatory (Clausen had been invited to Munich by the optical establishment of Utzschneider). In this situation he carried on the current observations of the Altona Observatory with great zeal and skill for twenty-four years, till Schumacher's death, and distinguished himself by the discovery of three comets. At the same time he took a share in many other scientific labours, which we owe to the Altona Observatory. In 1829 and 1830 he made the astronomical observations for the pendulum experiments at Güldenstein, and afterwards worked for many years on Schumacher's arc of the meridian. He connected the Amager base, measured with Bessel's apparatus, with the triangulation. He also connected Schumacher's triangulation with that of Sweden, and took a considerable share in its connexion with that of Prussia.

Bessel has given a striking proof of his reliance on Petersen's scrupulous trustworthiness. This great astronomer, shortly before his death, specially directed that Petersen and Busch should complete the reduction of the declinations of the fundamental fixed stars, which he had observed with the Repsold Meridian Circle, and in the way in which he himself had already commenced the work. In consequence of this honourable commission, Petersen visited Königsberg to bring away the necessary papers. The Philosophical Faculty of Königsberg took this opportunity of presenting him with a doctor's diploma. These reductions were completed before Petersen's death, and the publication may be looked for from Professor Busch, the director of the Königsberg Observatory, at no very distant time.

After Schumacher's death, Petersen held, temporarily, the direction of the Altona Observatory, and continued the publication of the *Astronomische Nachrichten*, at first alone, but afterwards in conjunction with Professor Hansen. In 1852 he was named by the King of Denmark a member of the commission for regulating the Altona Observatory, and received the title of Professor. He died at Altona on the 3d of February, 1854.

Petersen, as is evident from the preceding notice, came to Altona with but little knowledge of mathematics or astronomy; but he strove with great zeal and great success to repair these

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deficiencies. The volumes of the *Nachrichten*, from the 7th to the 37th, give abundant evidence of his astronomical proficiency, especially in the departments of observation and calculation. He married Sept. 2, 1832, but left no family. His private life was in the highest degree peaceful and happy, and his upright and unpretending character won the respect of all who knew him.

Felix-Victor Mauvais was born at Maiche, a village in the Department of Doubs, France, on the 7th March, 1809.

He was sent early to the seminary of Besançon, and there laid the foundation of a good education. The desire of acquiring more extended knowledge induced him to proceed to Paris, where, after pursuing his studies with assiduity in the various schools of learning of the capital, he was admitted as a student at the Ecole Polytechnique. Monsieur Mauvais afterwards gave instruction in the Mathematics at the Institution of Monsieur Barbet.

About this time he received a gratifying mark of the esteem in which he was held in his native Department by being comprised in the number of young men adopted by the town of Besançon, when he received a pension on the Suard Foundation, which afforded him additional means for pursuing his studies.

His taste for astronomy and for the exact sciences generally rendered him desirous of being attached to the Observatory, to which he was appointed by the Bureau des Longitudes in 1836, at the recommendation of Monsieur Arago.

Finding himself permanently attached to the Observatory, and at liberty to follow his favourite scientific pursuits, he did not limit his labours to an active participation in the ordinary duties of the establishment, but entered with ardour upon various astronomical investigations.

His zealous exertions were rewarded by the discovery of four comets.

During the eighteen years that Mons. Mauvais belonged to the Observatory he was indefatigable in making observations, and acquired much distinction as a practical astronomer, so that his labours added much to the high reputation of that establishment.

The talents and industry displayed by Mons. Mauvais caused him to be appointed a member of the Institute of France. He was named in 1843 to succeed Mons. Bouvard in that learned body; and at the end of the same year he was selected as an astronomer to the Bureau des Longitudes.

His varied acquirements and his thorough acquaintance with every part of practical astronomy caused his opinions to be much valued at the meetings of this body. Mons. Mauvais was not less distinguished for his amiability and independence of character than for his scientific attainments.

His health began to fail under his continual labours: however, his love of science was too great to allow him to seek renewed health in tranquillity and a cessation from astronomical studies.

This distinguished astronomer died in March 1854, at the com-

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paratively early age of forty-five, much regretted by his numerous friends and by those who looked forward to further contributions to science from so intelligent and so energetic an astronomer.

Capt. Francis Price Blackwood, R.N., was the second son of Vice-Admiral the Hon. Sir H. Blackwood. He entered the Navy in 1821, at the age of thirteen, and was promoted to the rank of captain in 1836. His activity and his taste for scientific pursuits pointed him out as well qualified to conduct the examination of part of the north-east coast of Australia and the adjacent seas, which were imperfectly known; and in the beginning of 1842 he sailed in command of H.M.S. Fly on that service. While visiting the island of Teneriffe he ascended the Peak and obtained a satisfactory barometric observation, which afforded a height agreeing very nearly with that of Humboldt: he also made a slight survey of the island of St. Paul's, in the Indian Ocean, which was published by the Admiralty. Capt. Blackwood made a detailed examination of many of the small groups of islands within the barrier reefs on the north-east coast of Australia, and of the "inner passage," or expanse of smooth water between these reefs and the coast.

This service, performed amongst coral reefs rising suddenly from great depths, was one of considerable danger, and proved that Capt. Blackwood possessed in an eminent degree the professional skill and personal qualities which are indispensable to a successful issue in such circumstances, and by which, combined with patience and good temper, he preserved his crew from hostile collision, except on two occasions, with the treacherous natives of Australia and New Guinea. Capt. Blackwood erected a beacon on Caine's Island,—a very important position to navigators in those seas.

Some occultations observed by Capt. Blackwood and computed by Mr. Breen will be found in the *Monthly Notices* of the Society. He also devoted particular attention to the measurement of some important differences of longitude.

An account of the voyage was published by Mr. B. Jukes, M.A., the naturalist of the expedition, by the authority of the Admiralty, under the title of *Narrative of the Surveying Voyage of H.M.S. Fly.*

Capt. Blackwood returned after this expedition to the ordinary course of service; but his career, on which his zeal and abilities would, it was hoped, confer lustre, was prematurely closed by cancer, of which he died, at the age of forty-five, much regretted. He has left a widow and two children.

Edward Riddle was born in 1788, at Troughend, in the county of Northumberland. At this place he received his early education, and afterwards attended a school at Otterburn, on Reedwater, a village not less interesting from its beautiful and romantic situation than from its historical reminiscences. At this ancient

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village Mr. Riddle commenced his professional career, and kept a school for a short period. Here he became acquainted with a very remarkable person, the late Mr. James Thompson, who, from his varied attainments in science and natural philosophy, was regarded as a "prodigy" by the rustic population of the village and neighbourhood. In his intercourse with this gifted man, it is not improbable that Mr. Riddle caught a spark of that enthusiasm for science which pervaded his character, and derived from him that taste for scientific pursuits which he retained to the end of his life. It is well known that he urgently pressed upon Mr. Riddle the absolute necessity of acquiring a thorough knowledge of the principles of geometry, and to take Playfair's *Euclid* as his guide, assuring him that it was "a capital book."

While at Otterburn, Mr. Riddle made an electrical machine with his own hands, and with it showed all the ordinary phenomena produced by that instrument: and at that period it may be easily imagined what wonder and alarm would fill the minds of a row of rustics when the electric impulse was made to dart through their bodies with a sensation never before experienced.

In 1807 Mr. Riddle removed from Otterburn to Whitburn, in the county of Durham, where he remained nearly seven years. Here he commenced to study science in earnest; and, acting on the wise counsel given him by Mr. Thompson of Otterburn, he first completely mastered the whole of Playfair's *Euclid;* and he has been heard to assert that the accomplishment of this task produced such an effect upon his mind as to render the acquisition of any other mathematical subject a matter of comparative ease. He now became a *Diary* correspondent, his first communications to that work appearing in 1810, dated from Whitburn. This periodical was at that time under the management of Dr. Hutton; and Mr. Riddle's mathematical correspondence speedily procured him the esteem and friendship of that distinguished mathematician, who rendered him important assistance in advancing his success in life. Though, at this period, Mr. Riddle was little more than twenty years of age, he had acquired a considerable knowledge of mathematical science, and for many years he continued to be a distinguished contributor to the *Diary*, in which his solutions were always remarkable for conciseness, elegance, and accuracy. In the years 1814 and 1819 Mr. Riddle obtained the prize given by the editor of that well-known periodical.

Through the recommendation of his friend, Dr. Hutton, Mr. Riddle was appointed, in 1814, Master of the Trinity House School, Newcastle-upon-Tyne. The nautical instruction in this school, at the time of Mr. Riddle's appointment, was in the lowest possible state; but, by his zeal and abilities, he speedily raised its character to a high standard. His fame, as a teacher of navigation and nautical astronomy, spread far and wide; he drew around him a large number of pupils, and gained the respect and esteem of a wide circle of friends and contemporary teachers, who, after he had left Newcastle, never ceased to remember him with feelings

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of the deepest regard. Here also he became noted for the surprising quickness and accuracy with which he took celestial observations.

In 1821, Mr. Riddle published an essay, entitled, "Observations on the Present State of Nautical Astronomy; with Remarks on the Expediency of Promoting a more General Acquaintance with the Modern Improvements in the Science among the Seamen in the British Merchant Service." This essay, which was dedicated to the Master and Brethren of the Trinity House, Newcastle, is admirably written, and reflects great credit on its author. From a series of lunar observations, taken in 1821, and recorded in this essay, the longitude of Trinity House School was found to be 1° 37′ 17″ W.

In 1821, by the same powerful influence of Dr. Hutton, Mr. Riddle was appointed Master of the Mathematical School, Royal Naval Hospital, Greenwich, where he remained to the time of his retirement in 1851. Soon after his removal to Greenwich he was elected a Fellow of this Society, and, in 1825, his name first appeared on the list of Council for that year. From that period to 1851 he was either one of the Council or a vice-president, and took an active part in every plan of the Society for the advancement of astronomical science. Mr. Riddle was always a regular attendant at the meetings of Council; and it is remarkable that he never attended one of these meetings without having previously obtained leave of absence from the Governor of the Institution. He was always so highly esteemed by the authorities of Greenwich Hospital, that he might have left his post on these occasions without the fear of censure, but his stern integrity, and unbending rectitude of character, would not suffer him to neglect the monthly presentation of his request; and it is needless to say that such leave of absence was never refused him.

Mr. Riddle contributed some valuable practical papers to the *Memoirs* of this Society. Among them is one "On the Longitude of Madras, by Moon-Culminating Observations," which he prepared at the request of the late Mr. Baily. This is an excellent paper, containing some valuable formulæ and remarks. It is printed in the twelfth volume of our *Memoirs*.

Mr. Riddle is best known by his valuable work on Navigation and Nautical Astronomy. He had been collecting materials for this work before he left Newcastle, but it was first published in 1824. It was an immense improvement on the empirical compendiums in vogue when it appeared, combining practice and theory in just proportions. Its leading characteristic is,—that while it contains all the tables and rules for computation necessary for the practical seaman, it contains also the investigations of the rules, and the preparatory mathematical information necessary for understanding these investigations. This is the crowning excellence of the work; and, as a proof of its admirable adaptation to the purposes of instruction, no less than five editions of the work have been disposed of, and the sixth, under the superintendence

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of his able and talented son and worthy successor, Mr. John Riddle, will shortly be published. It is hoped that the forthcoming edition will be found to be one of the best books for nautical instruction that can be put into the hands of seamen.

In 1822, Mr. Riddle communicated a short paper to the *Philosophical Magazine* "On the Simplification of Ivory's Solution of the Double Altitude Problem." By a very simple trigonometrical transformation, he adapted Ivory's solution to logarithmic computation, and gave to it that practical working form now in use.

In 1841, the school which Mr. Riddle had so long conducted was divided, Mr. Riddle retaining the superintendence of the senior portion, which was then called the Nautical School. The mistaken zeal of some persons in the cause of education led to the appointment of a very incompetent person* to the mastership of the junior portion of the school, who, thoroughly ignorant of the heavy and much self-imposed labour in which Mr. Riddle had so long delighted, proved a source of much anxiety and annoyance. Assertions were made to the Board of Admiralty, in reference to the progress of Mr. Riddle's pupils, which induced the Board to request two public teachers of mathematics to visit the school and report on the state of the instruction. The report made by these gentlemen completely refuted the assertions, and strongly expressed to the Lords of the Admiralty the opinion of the reporters, that the state of the school was not only highly satisfactory in itself, but, considering the low state of instruction of the pupils at entrance, and the small number of teachers allowed, a very remarkable proof of Mr. Riddle's energy and ability. It will be unnecessary further to revert to these circumstances — peculiarly painful to a sensitive mind like Mr. Riddle's — than to state that they resulted, if possible, in a higher estimation of him by all who knew him, and added not a few to the list of his friends. Something of disappointment, however, remained; neither was there from that time the same elasticity and cheerfulness which had marked his earlier career.

In 1845, a most able and efficient staff of masters had been happily gathered around him, and they united to present him with a testimonial of their esteem. On that occasion the Governor, Admiral Stopford, complimented Mr. Riddle on the firm stand he had made under some recent difficulties which had then happily passed away.

Shortly after Mr. Riddle's retirement in 1851, his bust in marble, finely sculptured by Mr. Theed, was presented to him by a large number of his former pupils and friends, accompanied with the expression of their high esteem for his worth, both as a public and private man. It was presented to him in the presence of the boys, and a large circle of friends and naval officers, includ-

* Since deceased.

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ing the Governor, Admiral Sir Charles Adams, who, in a very feeling and effective address, adverted to Mr. Riddle's long, useful, and honourable services in the Institution.

Mr. Riddle was allowed to retire on his full salary; but he did not live long to enjoy his cessation from active duty, as successive attacks of paralysis issued at last in the complete prostration of his physical powers. His death took place at his residence in Greenwich, on the 31st day of March, 1854, in the sixty-seventh year of his age.

Mr. Riddle was distinguished among the many mathematicians who have done honour to the county of Northumberland; he was much esteemed by all who enjoyed the privilege of his acquaintance; his habits were plain, unobtrusive, and unostentatious; his hospitality and obliging disposition will be long remembered by those who have experienced them; and his attachment to his friends was warm and sincere. He was partial to music; at one time he could perform very pleasingly on the violin; and his taste for the songs and music of Scotland, as well as the Irish melodies and Welsh airs, led him to purchase every work of this description on which he could lay his hands.

Mr. Riddle's success as a teacher of navigation and nautical astronomy has never been surpassed—nor even equalled; he brought to his task a thorough acquaintance with the minutest details of the subject he had to expound, and his activity of manner and energy of character carried him through almost any amount of labour. Nor did his toils always close with the day; at midnight, or in the early morning, he would frequently repair to the dormitories of the boys, and rouse the elder ones, who were accustomed to take observations of the moon or other celestial visitant, with the quick call of "*my first class.*" Thus day and night witnessed his unwearied exertions and unparalleled industry in the dissemination of both theoretical and practical science among the youth attending the Royal Hospital Schools. He was one of the most indefatigable teachers of the age: the high status which these schools have so long maintained, in preparing youths for sea-service, is almost entirely attributable to his exertions and example; and there are very many masters and commanders in the Royal Navy, as well as officers in the Merchant Service, in every part of the world, whose naval services are at this time testifying to the soundness and efficiency of their early education derived from Mr. Riddle of Greenwich. No teacher ever deserved the gratitude of his country more than Mr. Riddle; few have done her more real and substantial service, and conferred so many advantages on her naval and commercial interests.

While the Council regret the loss of the services of Mr. Riddle, who was one of their number for twenty-five years, they have the pleasure of congratulating his son on his succeeding to the position of Head Master of the Nautical School; and they trust that with his high mathematical attainments and professional abilities,

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and stimulated by the example of his father, his exertions in promoting the cause of nautical instruction will be equally successful and distinguished.

ST. ANDREW ST. JOHN was the second son of the Rev. Edward Beauchamp St. John, and great-grandson of Henry Lord St. John, of Bletshoe in the county of Northampton. His early education was conducted by his father until he was placed at the Plymouth New Grammar-School, under the superintendence of W. Bennett, M.A., of Trinity College, Cambridge. Here he was prepared for admission to the Royal Military Academy at Woolwich, which he entered in February 1843. During his rapid progress through the Academy he displayed mathematical abilities of the highest order. His career was most brilliant, and it is believed almost unequalled. At the final examination he far outstripped all his competitors in mathematical attainments, and obtained his commission as a Second Lieutenant in the corps of Royal Engineers.

In December 1846 he was appointed to a command at Templemore, in Ireland, and was removed from thence to Gibraltar in 1847, and in the following year to Hong Kong, returning to England in August 1851. He obtained leave till the following January, when he went to Portsmouth and Hurst Castle. On the 17th December, 1853, he sailed on the Darien Expedition, the exposure and fatigue of which injured his constitution, and he returned the following spring much impaired in health, and was appointed to a command at Devonport, but was obliged to obtain leave on a sick certificate, and was never able to return to his professional duties. His death took place at Plymouth, 21st September, 1854, at the early age of twenty-seven.

Lieutenant St. John was passionately devoted to the study of mathematics, and whenever his military duties would admit, his leisure hours were spent in the prosecution of his favourite pursuits. Not a month before his death, he told the writer of this memoir, that, if he could obtain three years' leave, his most ardent wish was to proceed to the University of Cambridge with the intention of taking a degree in mathematical honours. His published writings are to be found in vol. i. (new series) of the *Professional Papers of the Corps of Royal Engineers.* They consist of a paper on the "Equilibrium of Roofs;" another on the "Equilibrium of the Arch;" and a third on "Dialling." He left many valuable papers unpublished.

WILLIAM SCOTT attained his position almost wholly by his own exertions. He was born in October 1800, at Maxton, in Roxburghshire, of a good family, but which then was in reduced circumstances. He had the misfortune to lose his father at an early age; but that father, while he lived, spared from his narrow income what was necessary to give his two sons, of whom William was the eldest, the best education which a country town afforded.

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At eleven years of age he was sent to the parochial school at Maxton, where he acquired a good knowledge of the English language and a fair amount of arithmetic; and three years afterwards he was sent to a school in the neighbouring town of Mertoun, in Berwickshire, in order that he might be instructed in the rudiments of classical literature, as well as extend his progress in arithmetic and algebra. Here he obtained a competent knowledge of Latin.

In November 1818 young Scott entered the University of Edinburgh, where he studied the Latin and Greek languages, besides logic, mathematics, and natural philosophy, under the distinguished professors of that time, among whom may be mentioned the names of Christison, Wallace, Leslie, and Jamieson, who honoured him with their particular friendship.

Mr. Scott began his mathematical studies under the tuition of Dr. Nicol; and the lessons of that mathematician he attended, as a private pupil, according to the practice in the Scotch Universities. By diligent application he speedily made the progress necessary to qualify himself for the mathematical class at the University, and was, in consequence, received there as a pupil of Professor, afterwards Dr. Wallace. A testimonial from Professor Wallace (1821) shows that he was one of the four students who shared the first prize for mathematics, given by the magistrates, patrons of the University. While at Edinburgh he supported himself, and paid the University dues, with what he could save from the remuneration he received for the lessons he gave to private pupils.

He was afterwards engaged at Edinburgh as an amanuensis by the late Capt. Basil Hall, R.N., whom he assisted in his astronomical pursuits; and, in preparing for the press the works of that officer, he greatly improved his style and increased his facility in English composition. In 1826, while so employed, he took his degree of M.A. in the University of Edinburgh; and, in the same year, he was recommended by his patron, Professor Wallace, on an application from the late General Butler, to fill the post of a Mathematical Master in the Royal Military College at Sandhurst.

The reputation of Professor Wallace was deservedly so high in that Institution, in which for many years he had been one of the mathematical professors, that the recommendation was immediately accepted, and Mr. Scott, being found duly qualified, at once received the appointment. He joined the Institution in February 1827. A reduction happening at that time to be made in the educational staff of the College, Mr. Scott was at first charged with the instruction of a class of gentlemen cadets in History and Latin; but, within a year, he entered upon his regular duty as a Professor of Mathematics, and this duty he continued ably to fulfil to the time of his death. The retirement of two professors, who were his seniors, placed him in a few years in the rank of first professor in the junior department of the Institution. In this situation all the energies of his mind

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were devoted to his duties as an instructor, and to the composition of his lectures.

Before this time the course of study in mathematics at the College must be considered as having been very elementary; and to Mr. Scott fell, in a great degree, the task of preparing the plan, and carrying out the measures adopted, for extending the course into the higher branches of mathematical science. In furtherance of this view he published, 1844, his *Treatise of Arithmetic and Algebra*, and, in 1848, his *Treatise on Plane Trigonometry and Mensuration*—works which, without, in matter or treatment, being beyond the mental powers of the youths for whom they were intended, have the great merit of being written in a philosophical spirit; and, had his life been prolonged, those works would have been followed by treatises on the higher departments of mathematical analysis. These he had in a great measure prepared, and in a manuscript form were in the hands of his pupils, who owe to him the knowledge they acquired of an important branch of human learning, and who cannot but remember with pleasure the general suavity of his manner, and the pains he ever took to remove the difficulties which beset the career of their professional education. Besides the works just mentioned, Mr. Scott, in 1854, published an elementary treatise on arithmetic, which is rendered particularly valuable by containing a series of elaborate tables computed for the purpose of facilitating the introduction and employment of a decimal scale of weights and measures. He was admitted a Fellow of the Royal Astronomical Society in 1835, but his distance from town, and the duties of his professorship, which required his presence almost daily at the College, rarely permitted him to attend its meetings; he took, however, the greatest interest in its proceedings, and in the discoveries which have added of late so much lustre to the science, for the promotion of which it was formed.

In 1852 Mr. Scott was appointed examiner in mathematics of the candidates for commissions in the British army, and this delicate post he held till his death, exercising the duties with strict impartiality, and to the entire satisfaction of those in authority.

Mr. Scott's constitution had never been strong; and in the beginning of last year (1854) his health began rapidly to decline. He died on the 8th of July; his complaint being aggravated by unremitting attention to his studies and the duties of his professorship.

Robert Snow was the eldest son of Robert Snow, the banker. He distinguished himself much at Eton and Cambridge, gaining especially Sir William Browne's medal for the best Latin ode in 1825. Since he took his degree he gave up many years to literary pursuits. Some publications, printed a few years since, prove how sincere an admirer he was of poetry, and how acutely he observed the beauties of the natural and celestial worlds. In the

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work called *Memoranda of a Tour on the Continent*, he gives much information during his expedition amongst the Alps, particularly during the passage of the Col du Géant; and his journey to Sweden to observe the total eclipse, some years ago, proves his zeal in the pursuit of astronomical researches. He also published a small compendium of astronomical definitions for the use of learners.

For the last few years a painful and distressing complaint prevented his application so earnestly to his favourite studies, and at last obliged him even to give up his observations at his observatory at Ashurst.

Mr. Snow was for a long time a Fellow of the Society, and was for several years on the Council. He was at one time a regular observer, and our volumes of *Memoirs* and *Notices* contain many of his observations. He was a remarkably nice noter of phenomena, and his occultations of stars by the moon may be safely relied upon.

George Burgess Wildig was born at Lichfield in the year 1784. His parents were inhabitants of Betley in Staffordshire. He received his school-education at the Grammar-School of Newcastle-under-Lyne. Though remarked as intelligent and studious, he left school at an early age, and entered a mercantile house at Liverpool, in which he at length became a partner. When about twenty-five years of age he abandoned business for pursuits in which he might more easily gratify the bent of his mind towards study and meditation. With this purpose he removed to Edinburgh in 1808, and for three sessions attended classes in that university, particularly those of Professors Dugald Stewart, Christison, and Leslie. By these eminent men he was especially noticed, and was honoured with their lasting friendship. His services were given in correcting for the press some of Professor Leslie's important works. In 1811 he entered at Caius College, Cambridge, where Professor Woodhouse appreciated his merits, and used his assistance, as Leslie had done at Edinburgh. He graduated B.A. in 1815, and then went to reside again in Liverpool, entered into holy orders, and held a curacy in that town. A short time afterwards he accepted the mathematical professorship at the Liverpool Institution, which office after a few years he resigned. In 1826 he was instituted to the Rectory of Norton-on-the-Moors, near Burslem, in Staffordshire, of which parish he previously possessed the advowson. His discharge of parochial duties was marked by attention, moderation, sound judgment, and consistency. His spare time was given to his favourite pursuit of mathematical science, blended with ancient and modern literature. With the Italian writers of the best ages, and with the niceties of their language, he was intimately acquainted. The rupture of a blood-vessel compelled him in the latter years of his life to resign clerical duties, and to make his abode in a mild climate. He chose St. Heliers in Jersey, where he died on the 9th of December, 1853.

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His attachment to mathematical science continued to the last, and he beguiled many of the hours of pain and weakness in the two years next before his death by an elaborate recension of every article in one of the treatises on algebra most in use in our universities. Though he has left much matter in manuscript, he rarely gave anything to the world through the press. A short geometrical treatise on the conic sections, which he printed in 1822 for the use of the Liverpool Institution, is marked by elegance, geometrical purity, and comprehensiveness. He was the author of some mathematical articles in the seventh edition of the *Encyclopædia Britannica*, the signature to which is O. O. O.; but in the list of contributors his name is erroneously given as the Rev. G. B. Wilding, M.A., *Oxford*. Many friends of similar pursuits, and many grateful pupils remain sensible of the advantages they have had from his luminous conversation and instructive guidance. His scientific strength lay on deep foundations. He was conversant with the Greek geometers, and with the discoverers, promoters, and perfectors of modern mathematical investigations from the middle ages to the present day. His remarkable talents and learning, though so little displayed to the world, were well known to many friends; among whom it is needless to mention those Cambridge men of his day, who were among the first founders, and are yet distinguished members of the Astronomical Society, and leading names in British science.

John William Whittaker, D.D., was born about the year 1790. He graduated at St. John's College, Cambridge, where he took his B.A. degree in 1814, his name appearing as thirteenth on the list of Wranglers for that year. He was subsequently elected a Fellow of his College; and it was while residing on his Fellowship that he published his celebrated work which introduced him to the notice of the late Archbishop of Canterbury. This was entitled "A Historical and Critical Enquiry into the Interpretation of the Hebrew Scriptures, with Remarks on Mr. Bellamy's New Translation." It was printed at the University press during 1819, and was followed shortly after by a "Supplement" containing numerous additions and corrections. In consequence of the publication of this work he was appointed Examining Chaplain for the Archbishop, who presented him to the Vicarage of Blackburn on the demise of the Rev. Thomas Durham Whitaker, LL.D., F.A.S., the well-known historian and archæologist, in 1822. He proceeded to the M.A. degree in 1817; was admitted B.D. in 1824; and completed the usual course for the degree of Doctor in Divinity during 1830. The offices of Rural Dean and Honorary Canon of Manchester Cathedral were also conferred upon him by the Bishop of that diocese.

During Dr. Whittaker's residence at Blackburn he published a series of letters on the church; and in 1825 he was appointed to preach a course of sermons before the University of Cambridge, the subject being justification by faith. This course

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was published the same year, and was appropriately dedicated to his patron the Archbishop. Besides these he also published various pamphlets on passing subjects of controversy; one of which was a sermon preached to the Chartists when they took possession of the parish church in 1839; and another related to the remains of the ancient British language in the northern part of the kingdom. Although his parochial duties necessarily occupied a large share of his attention, he always kept up his scientific and general reading. Not long before his death the writer of this notice had an opportunity of hearing him explain his researches into the affinities of the Chinese language so far as they had a bearing upon the cosmogony of the first chapter of Genesis; and he listened with delight to his examination of the different systems of computing time adopted by our leading geologists. He had elaborated, to use his own expression, a theory of the universe, which, in his opinion, tended to remove all difficulties, and reconciled the biblical, the astronomical, and the geological theories. Had his health permitted, he would undoubtedly have favoured the world with some of his researches concerning the nebular hypothesis and geological time. He was one of the first Fellows of the Astronomical Society, having, as the writer understood, assisted in its formation. He always entertained a high opinion of the value of the labours of the Society, but regretted that other urgent duties had almost isolated him from subjects in which he formerly took a deep interest.

As a preacher Dr. Whittaker never aspired to the character of a popular orator; but the calm and argumentative style of his sermons was calculated rather to correct the judgment of his hearers than to rouse their feelings by appeals to the imagination. As a writer he had few superiors in the purity of his diction and the rhythmical composition of his sentences. His health had been failing for some years, and he finally sank under a complication of diseases, at the Vicarage, on August 3d, 1854, in the 64th year of his age. The public of Blackburn honoured his remains with a public funeral a few days after his decease, the Lord Bishop of Manchester officiating at the ceremony.

Captain Sir JOHN FRANKLIN, of the Royal Navy, Knight Commander of the Guelphic Order of Hanover, D.C.L. and F.R.S., was an early Fellow of this Society, and must be well remembered by many of those who are now present. He was born at Spilsbury in Lincolnshire, in the year 1786, entered the navy at the age of fourteen, and was on board the Polyphemus at Lord Nelson's attack on Copenhagen in 1801. Shortly after that memorable action he joined the Investigator sloop, Captain Matthew Flinders, and sailed on a voyage of discovery to New Holland; and it was under that regretted officer that he imbibed that zeal for geographical research for which he was distinguished through life.

While thus engaged, he had the misfortune to be wrecked on a coral bank; and in returning home on board an East-Indiaman,

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he was present at the celebrated repulse given to a French squadron under Admiral Linois, by Commodore Dance and his merchant-ships, on which occasion Franklin was one of those who managed the signals which so greatly deceived the enemy. On arriving in England he was immediately appointed to the Bellerophon, of 74 guns, in which well-known ship he bore a part in the great battle of Trafalgar, on the 21st of October, 1805. He served with great merit during the remainder of that arduous war, and was strongly recommended for promotion for his conduct at New Orleans.

On the general peace in 1815 his mind reverted to its former bent, and he bestirred himself in the cause of discovery. In January 1818, he assumed command of the hired brig Trent, and accompanied Captain David Buchan on a voyage of research to Spitzbergen, which led to his being intrusted with the charge of an expedition to the Coppermine River and the northern regions of America. The details of that fearful undertaking, together with his subsequent polar explorations, are too widely known to need repetition here; as is also having been some time the Lieutenant Governor of Van Dieman's Land. Suffice it to say, that he sailed in the Erebus on the 3d of March, 1845, on his last and ill-fated voyage. Since then the public feeling has been painfully excited respecting the fate of our hapless countrymen in those inhospitable wastes, and the most liberal and spirited measures were adopted, as well by our Government as that of America, and by the bereaved Lady Franklin, for their rescue; but an unusually dense mystery pervaded the whole.

The hope which glimmered faintly in the gloomy uncertainty has at length been quashed by the recovery of certain unmistakeable articles belonging to Franklin and the officers of the Erebus; and yet even this requires further evidence. The plate, the clothing, and the decorations of knighthood, which have been brought to England by Dr. Rae, form a conclusive token of a terrible catastrophe having happened to that ship; but as Dr. Rae is confessedly unacquainted with the Esquimaux language, and only gained his information through the equivocal medium of an interpreter, we must pause upon several points of the tale he has published, and especially the distressing statement that our people resorted to cannibalism. It must be borne in mind that the period of the tragedy is stated to be in 1850,—that the natives communicated with were not those who had communicated with the Franklin party,—and that there would be an obvious object in their misleading Dr. Rae. Notwithstanding, however, the accounts being unsatisfactory, there can be no reasonable doubt of the extent of the disaster. The Council have not ventured to erase the name of Sir John Franklin from our list until the Admiralty set the example: and it is with feelings of deep sorrow that they acknowledge the propriety of such a step.

Captain Francis Rawdon Moira Crozier was born at Bain-

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bridge, in the county of Down, in Ireland, and was early destined for sea life. He entered the navy in June 1810, on board the Hamadryad frigate, under the command of that active officer, Sir Thomas Staines; whom he afterwards accompanied to the Pacific in the Briton, and made an interesting visit to Pitcairn's Island, which they found peopled by the mutineers of the Bounty. In 1824, Mr. Crozier was appointed master's mate of the Fury, discovery ship, under Sir Edward Parry, with whom he became associated in three successive polar voyages; having been made a Lieutenant in March 1826.

Mr. Crozier was afterwards employed on the coasts of Spain and Portugal, till December 1835, when he joined the Cove, a hired vessel, which was despatched from the Humber in search of certain missing whalers, under the command of Sir James Ross. Being now distinguished for science, seamanship, and fertility of resource, his services came into demand; and in May 1839 he was appointed to command the Terror, in which ship he accompanied the same officer on a voyage of research into the Antarctic Ocean, during which time he was advanced to post rank. In March 1845 he was re-commissioned to the Terror, and sailed on a fresh attempt to explore the north-west passage, through Lancaster Sound and Behring's Strait. He has not since been heard of!

Though the articles brought to England by Dr. Rae were mainly illustrative of the fate of the crew of the Erebus, there is every apprehension that Crozier and his gallant associates shared the fate of their commander. As the seas and land on the meridian of Cape Walker, and north of it to a distance exceeding 100 miles, have been thoroughly searched for the missing ships without success, it is not improbable that they are frozen up in one of the channels between North Somerset and Banks's Land, which have not been yet explored. It is therefore just possible that a few of the juniors, with hardy constitutions, may survive; but the seniors must, long ere this, have succumbed to privation and suffering.

The operations at the Royal Observatory, Greenwich, have been characterised, during the past year, by the same activity that has prevailed in former years. No important addition has been made to the instruments; but those already in use have been employed with the same inflexible steadiness and on the same classes of objects. It was stated in the last February Report, that the mechanism necessary for the registration of transits, by means of electro-magnetism, was nearly complete; and we may now add that the method has been for several months in use with perfect success and with considerable advantage to the observations. The first transits recorded by this method were made on March 27, 1854; and since that time all the transits, with trifling and occasional interruptions, owing to derangements of the machinery, have been made by the same agency. The observations made over nine wires by this method are indisputably

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of a higher order of excellence than those formerly made by the ordinary method of the eye and ear; and great hopes are held out that the personal equations of the different observers will be comprised within very narrow limits.

The only important addition to the external galvanic communications, as they were reported last year, is that arising from the completion of the arrangements for dropping a time-signal ball at the Navy Yard at Deal. The dropping of the ball systematically came into operation at the commencement of the present year, and with perfect success as far as the general action of the machinery is concerned, though several failures have arisen through defects in the local galvanic clocks, and from other causes. All these defects have since been remedied, and there is no doubt of the facility with which the dropping of the ball can be ultimately effected as a matter of routine business; and there cannot be a doubt of the great advantage which will accrue to this important port by the correct knowledge of Greenwich time.

The arrangements for determining the difference of longitude between the Observatories of Greenwich and Brussels were stated with some detail in the last February Report. The result was there given that was deduced from the first part of the operation, namely, a difference of longitude amounting to $17^m\ 29^s{\cdot}256$. We may add in this place that the second part of the operations, namely, that after the interchange of the observers, gave, for the resulting difference of longitude obtained by the same mode of treatment, $17^m\ 28^s{\cdot}538$; and that the results corresponding to a rather different mode of treatment of the star-observations, were respectively $17^m\ 29^s{\cdot}340$ and $17^m\ 28^s{\cdot}476$, as deduced from the two sets of operations, and that the whole number of signals on which these results are based was 1104. The definitive result of the whole series of operations is $17^m\ 28^s{\cdot}90$, which differs by $1^s{\cdot}3$ from the result previously deduced by M. Quetelet and Mr. Sheepshanks by means of chronometers. We may also remind the Society that a paper by the Astronomer Royal, which explains with full detail every part of the operations, has been printed in the current volume of our *Memoirs* recently commenced.

But the most important series of operations for the determination of longitude performed during the past year, is that by which the difference of longitudes of Paris and Greenwich has at length been definitively settled. It is well known to the Society that this determination was intended as one of the earliest applications of the galvanic method; and, as early as the year 1851, a correspondence was commenced by the Astronomer Royal with M. Arago on the subject. Various causes contributed to the delay of the projected operations, amongst the most prominent of which were the ill-health and subsequent death of M. Arago, and the consequent unsettled condition of the administration of the Paris Observatory; as also the failures which occurred soon after the completion of the submarine galvanic connexions with the Continent, and the delays in establishing the connexion of the Observ-

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atory of Greenwich with the line of wires of the Submarine and European Telegraph Company.

Owing to all these causes, the operations connected with the determination of the longitude of Paris did not commence till the month of May 1854, but they were then carried through with a completeness and accuracy superior to all preceding attempts at the determination of longitude. Independently of the care bestowed at Greenwich on every part of the operations, the successful issue of them was due mainly to the energetic way in which M. Le Verrier entered into every part of the details of his share of the work, and surmounted the successive difficulties which lay in his way.

It has been proved, with regard to earlier attempts at determining longitudes by other methods, that, generally speaking, the doubtful or failing point has been in the determination of local time at the different stations. However well rocket or other signals may be observed, and how skilfully soever all the details of the operations may be conducted, yet any errors affecting the time as obtained by the transit instrument, whether from faulty construction, bad determinations of instrumental errors, or uneliminated personal equations of the observers, are fatal to the whole operation.

On entering upon the Directorship of the Paris Observatory, M. Le Verrier found his transit instrument very defective, and utterly unfit, without great alterations, for the delicate work required of it. With his characteristic energy he proceeded immediately to remedy this serious defect; and, though he was obliged to put up with considerable delay, he at length got the transit instrument reinstated in a perfectly satisfactory condition. To this cause, as well to his minute attention to the instrumental errors during the whole course of the operations, is mainly due the confidence which we repose in the final determination.

With regard to the operations themselves, they consisted, as in former instances, of two series; in the first of which M. Faye, on the part of the Paris Observatory, observed stars and galvanic signals at Greenwich, while Mr. Dunkin, on the part of the Greenwich Observatory, made the similar observations at Paris. In the second series, the observers were reversed — M. Faye observing at Paris, and Mr. Dunkin at Greenwich.

In the first series, the observations commenced on May 27, and terminated on June 4, 1854; five evenings during that interval being made available for the determination of longitude by the observation of transits of stars as well as galvanic signals at both observatories. In the second series, the observations commenced on June 12 and terminated on June 24, seven evenings being available for longitude. The determination from the first series, as derived from more than 700 signals, was that the east longitude of Paris is $9^m\ 20^s{\cdot}50$; and that from the second series, as derived from nearly 1000 signals, gave for the east longitude, $9^m\ 20^s{\cdot}76$. The definitive result is, therefore, $9^m\ 20^s{\cdot}63$, differing

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by nearly one second of time from that formerly determined by means of rocket signals.

In the course of the last summer, an important series of experiments was instituted by the Astronomer Royal for determining the variation of gravity on descending to a considerable depth in a mine, with the ultimate view of inferring from the result of this observation the mass, or the mean density, of the earth. Without referring to the indirect determinations of the law of density from the figure of the earth and the motions of the moon, the direct determinations hitherto made use of are of two classes,—one the attraction of Schehallien; the other the attraction of leaden balls, as observed by Cavendish, Reich, and Baily. In both these methods there are peculiar difficulties, and it appeared, therefore, at least desirable that a trial of a different method should be made. And it appears on calculation, that the probable effect of unavoidable errors when the mean density is inferred from experiments in a mine is less than in the other methods. These considerations induced the Astronomer Royal many years ago, in conjunction with Dr. Whewell, Mr. Sheepshanks, and other friends, to institute pendulum experiments in the Dolcoath mine in Cornwall. In two different years the experiments were tried, and in both they failed in consequence of local accidents. In the experience, however, of these abortive attempts, the observers learned that the principal inaccuracies arose from the difficulty of comparing a clock at the surface of the ground with one at the bottom of the mine. In the year just past, the observers at Greenwich acquired considerable practical familiarity with the manipulations of the galvanic telegraph; and it became evident to the Astronomer Royal that by its agency the difficulty, so formidable in other years, would now be easily overcome. Accordingly, he proceeded in the summer to inquire into the circumstances of different mines in the great Durham coal-field, and he found that the Harton Colliery, near South Shields, reported to be 1260 feet deep, was admirably adapted to the experiments, and that the owners of the mine were anxious to give every assistance. An expedition on a competent scale was soon prepared. It was necessary to obtain the assistance of observers from several English observatories. The working party, as finally arranged, consisted of Messrs. Dunkin and Ellis from the Royal Observatory, Mr. Rümker of the Durham Observatory, Mr. Pogson from Oxford, Mr. Criswick from Cambridge, and Mr. Simmonds from the Red Hill Observatory. Pendulums were lent by the Royal Society. The labour and skill of telegraph engineers, required in establishing simultaneous galvanic signals between the upper and lower stations, was gratuitously supplied by the liberality of the Electric Telegraph Company. The Admiralty assisted by a grant of a sum of money. The observations consisted of 104 hours of incessant observations of one pendulum (A) above and another (B) below; then of 104

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hours with (B) above and (A) below; then of 60 hours with (A) above and (B) below; then of 60 hours with (B) above and (A) below. The result, as to the mechanical firmness of the instruments and trustworthy character of the observations, is most satisfactory. The first conclusion is, that the lower pendulum is accelerated 2^{s}·25 per day, or that gravity is increased at the lower station by $\frac{1}{1900}$ part. Operations are now in hand for the precise measure of the depth of the mine, for ascertaining the general form of the country, and for obtaining the specific gravity of the rocks; and till these are completed the final result for the earth's mean density cannot be obtained. It appears, however, likely to prove high,—between six and seven times the density of water.

The publication of the thirteenth volume of the *Radcliffe Observations* has been already mentioned in the *Monthly Notices*. It contains the continuation of the observations for the circumpolar catalogue to the end of 1852. The *Observations* with the *Heliometer*, the publication of which was unavoidably delayed, are now printed, and the results are in the hands of the Society. They will appear, with the rest of the fourteenth volume, in the course of a few weeks.

It was mentioned in the last Annual Report, that they had been principally directed to the determination of the parallax of 61 *Cygni* and 1830 *Groombridge*. The partial result there mentioned, which gave a parallax of 0″·384 to 61 *Cygni*, has been fully confirmed. Subsequent investigation, taking in a greater number of observations, having only altered that value to 0″·392 or 0″·402 (accordingly as a temperature correction is used or not), with a probable error of ± 0″·015.

The observations of 1830 *Groombridge* have presented greater difficulties, and apparently an anomalous result, inasmuch as they assign a greater parallax to one of the stars of comparison than to 1830 *Groombridge*. Dr. Wichman, it will be remembered, fell upon a similar difficulty; and what adds to the perplexity is, the star which appears to have the greater parallax in this case is one of those which in Dr. Wichman's researches appeared to be most distant.

This is not the place to enter into a disquisition on the subject, but a short statement of the circumstances may not be uninteresting to the Society.

The stars selected for comparison with 1830 *Groombridge* are the same which Otto Struve used, but they have been designated in contrary order, the star which he called *b* being the *a* of the present inquiry. The same stars were also observed by Dr. Wichman in his more recent researches, in connexion with a *third* star. The two stars now in question are Wichman's *a* and *a″*. The comparative measures between 1830 *Groombridge* and the former give a parallax of 0″·26 to 1830 *Groombridge;* whereas in the other case the resulting parallax is −0″·18, or, in other

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words, the parallax of Wichman's a'' (the b of the present inquiry) is $0''{\cdot}44$.

Dr. Wichman found the parallax of 1830 *Groombridge* $0''{\cdot}72$, and that of his a (which is also a in this case) $1''{\cdot}17$ (*Astron. Nach.* No. 844, p. 50).

It were vain, in the present state of our knowledge of the structure of the heavens, to attempt to argue on the abstract probability of results such as these. It will appear to many minds just as improbable that a star having a proper motion of $7''$ of the great circle *should* be immeasurably distant from us (for this is the alternative), as that another having no physical peculiarity *should not* be.

It is not on such argument that Mr. Johnson founds his suspicion of the accuracy of the result at which he has arrived, but on the simple fact of two observers, with instruments similar in principle, though somewhat different in construction, having arrived at such opposite conclusions.

In his opinion, this seems to show some imperfection in double-image measures of objects separated by large arcs, which still remains to be explained.

It is true Dr. Peters has shown most ingeniously (*Astronomische Nachrichten*, No. 866) that Wichman's adopted temperature correction is probably too small. But he has not shown that the amended correction will reconcile independent results, so that each star separately will tell the same story; and until this has been done, Mr. Johnson conceives that his objection is not refuted.

The strongest argument in favour of Mr. Johnson's result, as it stands, is, that the observations of 61 *Cygni*, where the comparison stars are more unfavourably situated with regard to distance, show no such anomaly; and both series were carried on, in great part, contemporaneously. The parallax of 61 *Cygni*, with regard to its stars of comparison, is a maximum in April and October, about the times of the mean temperature of the year; while the maximum parallax of 1830 *Groombridge* occurs in December and June, at the extremes of annual temperature. Any disturbance, therefore, produced by expansion might affect the latter determination without affecting the former. But it happens that, of the *four* independent cases which have come under discussion, *one* only has shown symptoms of such disturbance, amounting to $-0''{\cdot}11$ for an increase of 10° of Fahrenheit on a distance of nearly $2100''$; while in *every* case the accordance of the separate results is improved by rejecting a temperature correction altogether. There was, however, this difference in the optical condition of the instrument in the two cases. In the case of 1830 *Groombridge*, in order to render the small stars visible when in juxtaposition to it, the aperture of one segment was reduced to about two inches, while with 61 *Cygni* no such reduction was necessary. Mr. Johnson is at a loss to explain *how* this circumstance can affect the accuracy of com-

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parative measures: it is mentioned as being the only apparent source of error which was not common to both series.

The observations for the Circumpolar Catalogue have at length been brought to a close. Some reasons for the delay which has occurred have been before mentioned. Besides these, another was the necessity of investigating into the cause of a small difference which has often been noticed between the N.P.D. given by the Radcliffe Observations and by other authorities.

Mr. Johnson has now satisfied himself on this point. A small excess of the adopted over the true latitude, amounting to about $+0''.35$, and an error in the adopted constant of refraction (Bessel's) of about $+0''.2$, will explain a portion of the difference. But the principal cause is the discrepancy between the horizontal points in different parts of the instrument, as given by direct and reflexion observations. Here we have another confirmation of the propriety of the warning often given to observers by the Astronomer Royal, not to trust to any single point for the determination of this important element of reduction. The effect of the accumulated errors will be to increase the N.P.D. about $1''.25$.

Considerable progress has been made during the last six months in bringing into action the photo-meteorographic instruments which had been some time erected. They consist of a barograph and thermograph, designed by and constructed under the superintendence of Mr. Ronalds, late of the Kew Observatory, to whose disinterested zeal and attention Mr. Johnson wishes to make this public acknowledgment.

The photographic process remained to be arranged, and for this purpose Mr. Johnson has been fortunate in securing the services of Mr. Crookes, a gentleman well known for his skill, and for several improvements he has introduced into the art. Under his direction everything has been very satisfactorily completed, and the observations are now being regularly carried on.

While on the subject of the Radcliffe Observatory, though not forming part of its strictly official routine, it would be unjust to pass unnoticed the personal labours of Mr. Pogson at intervals when released from his immediate duties.

Observations of the smaller planets at important parts of their orbit with the 10-foot equatoreal and ring-micrometer,—the determination of the periods of several known variable stars, and search for new ones,—the construction of a set of maps to connect Mr. Bishop's with those of the Berlin Academy, are subjects which engage his attention as circumstances allow. His progress cannot, of course, be that of a person exclusively devoted to this kind of research. But by system and perseverance he has accomplished a great deal, and it is hoped that no long time will elapse before the Society is put in possession of some of the fruits of his exertions.

Mr. Pogson was one of the observers whom the Astronomer Royal selected to take part in his important experiments at South

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Shields. These experiments afford, as far as we are aware, the first example of the mutual assistance one observatory may render to another,—an example which, we are sure, will be hailed with satisfaction by every astronomer. Apart from the direct advantages which accrue to men of kindred minds from the interchange of ideas, such intercourse between separate establishments serves to extend their sympathies beyond the sphere of local operations, and to remind them, while labouring each in his special vocation, that they are also members of an astronomical commonwealth, in the progress of which they have a direct concern.

At Cambridge, Professor Challis is proceeding steadily with the usual work. He has recently gone through a series of observations and calculations for determining the effect of the forms of the transit-pivots on the calculated times of meridian transit. Two sets of measures were taken, on different days, with the micrometer microscopes provided for bisecting dots at the ends of the pivots, the transit-axis being in one position, and two sets were similarly taken after reversing the instrument. The result of the calculations exhibited an effect of the same kind as that found by the trials made in 1850, but greater in degree. By the experiments of 1850 and 1854 the effect of the forms of the pivots on all the transit observations from the year 1850 inclusive, to the present time, may be eliminated. The more immediate object of the experiment of 1854 was to correct for the forms of the pivots, the determination of the longitude made last year by galvanic signals. Professor Challis was unwilling to publish a final result till this source of error had been got rid of. The calculations for this purpose, which were of great length, are now completed; and the final determination of the longitude by this method is $22^{s}.70$ east of Greenwich, which is about eight-tenths of a second less eastward than the longitude hitherto used. Professor Challis considers this result to be trustworthy, and proposes to adopt it in future. A detailed account of the galvanic experiment, and of the calculations connected with it, has been submitted by him to the Cambridge Philosophical Society.

Last summer he was engaged upon a plan for mounting collimators for the purpose of determining the effect of flexure in observations with the mural circle. As this instrument is of large dimensions, being 8 feet in diameter, the effect of the alteration of form caused by the mere weight of the parts is of sensible amount. He has hitherto attempted to eliminate this source of error by the comparison of direct and reflexion observations of the same star. This method, though generally good, fails, or becomes uncertain, when the stars are less than 25° above the horizon. On this account he proposes to use collimators so mounted as to be capable of collimating with the circle telescope in any position of the latter, being carried about the circle by opposite arms which revolve round a horizontal axis. The stage for mounting the collimators is prepared, and the collimators have been received from

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Mr. Simms, so that this method will soon have a trial. If successful, it will supersede the necessity of reflexion observations, which occupy a great deal of time, and are troublesome on account of failures arising from disturbances of the mercury. In any case, however, reflexion observations will be taken of stars near the zenith, in order that the determination of zenith point may not depend on a single set of divisions of the circle, which would happen if the zenith point were determined exclusively by the collimating eye-piece.

Steady progress has been made in the reduction of the meridian observations of stars near the elliptic, contained for the most part in the *Histoire Céleste*, and in Weisse's Catalogue reduced from Bessel's Zones. A considerable number of errors has been detected in these two catalogues, and some also in the catalogue of the British Association.

Mr. Carrington has during the past year been steadily pursuing the plan of observation proposed for his Observatory at Red Hill which was put forth in the *Monthly Notices*, vol. xiv., No. 1. Of the region comprised in the maps which he presented to the Society, the portion within 4° of the north pole has been exclusively under observation during 1854; and, with the exception of a mere trifle, is now exhausted. It was found that about one-sixth of the stars laid down in the maps were too faint to bear the slight amount of illumination necessary for the wires to be seen with any sharpness, and these have, consequently, been struck off the observing list. The remainder, which is believed to comprise all the stars not below the $10\frac{1}{2}$ magnitude, numbers 709 stars, of each of which, with very few exceptions, four observations, at least, have been procured in both elements. There are nineteen stars observed within 40′ of the pole, of which the nearest, situated in about 16^h by 0° 5′, may be accepted as an example of his lowest magnitude, the $10\frac{1}{2}$.

The companion to *Polaris* is rated at rather higher than the tenth. The reductions are also steadily progressing; but, from the additional labour required in the neighbourhood of the pole, are not likely to be concluded before Midsummer next. His transit-circle has given him the greatest satisfaction; but although he has evidence enough before him to show that its permanence of adjustment is nearly unrivalled, he thinks it better to defer any statement on the point till his reductions are more advanced. In the ensuing year, the zone 4° to 7° will be under observation, and, it is hoped, will likewise be exhausted within the year.

Throughout the past year the solar spots have been observed in position and sketched on every available opportunity. The method of observation is that which was explained in vol. xiv., No. 5, of the *Notices;* but the form of reduction has been modified in a manner which will shortly be published. It is sufficient now to state that the sun has been thus viewed during 1854 on 153 days, on thirty of which no spots were visible; and that the

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number of observations of nuclei and detached spots is 328, the whole of which are finally reduced and diagrammed, showing the appearance on the disk, and the heliographical longitude and latitude, the latter quantities computed to the nearest minute of arc. Mr. Carrington has not hitherto found time for their special discussion to any extent beyond this point. He intends to continue their observation and reduction on the same system during the present year. Mr. Carrington has likewise, from time to time, during the year, been engaged in the revision of the observations taken by him at Durham; and as the printer has some time since received the whole of the copy for the press, and about half the sheets are worked off, the Society may shortly expect to receive the second volume of the Durham Observations, which will be about the size of the first one.

The principal work of the Edinburgh Observatory during the past year has been the making, computing, and printing of the ordinary meridian observations of stars.

The several steps alluded to in our last Report for enabling the Observatory to undertake a larger amount, and a more special department of business, though advanced a stage further with the authorities, are not yet confirmed and allowed. But during the past year particular questions have been examined into as follows: the details will appear elsewhere.

1. The condensation of the luminiferous ether in the neighbourhood of the sun. The probability of such a condensation having been pointed out last spring by Prof. W. Thomson as resulting from the dynamical theory of heat, Prof. Piazzi Smyth proceeded to institute observations to test the fact, and measure its amount. The operation proved very tedious on account of atmospherical difficulties preventing the visibility of stars in the immediate neighbourhood of the sun; and, in fact, only two observations have been procured of a class fit to be employed in so delicate an inquiry. That these two observations unite in showing the fact of such a condensation, and give for the amount of it at 12° distance from the sun; one of them, ·03 sec., and the other, ·04 sec., is, Prof. Piazzi Smyth thinks, the result of accident; but he also thinks that we may safely conclude from them that while the quantity is certainly small, it may be sensible to a long series of good observations.

2. The physical character of the surface of the Mare Crisium in the moon, at the request of the British Association.

3. The application of electric agency to improve the accuracy of time observations.

4. The improvement of astronomical observations at sea, under circumstances to which the principle of Hadley's quadrant does not apply.

At the Liverpool Observatory, Mr. Hartnup's attention has of late been directed towards obtaining photographical pictures

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of the moon. At our June meeting, several specimens were exhibited to the Society, which were exceedingly promising, notwithstanding the imperfection of the apparatus for displaying them. At the meeting of the British Association at Liverpool, photographs of the moon were shown in St. George's Hall, which are said "to have outstripped all other attempts made elsewhere," and "to have superseded all maps of the moon now in existence." Mr. Hartnup does not think that, *at present*, his pictures quite deserve such praise; but he feels great hopes that they will do so before long.

The sensible defect is apparently in the *chemical* part of the process, for he finds that when the picture is magnified from 1·3 inches to 50 feet diameter, there is no fault which can be traced to the *motion* of the telescope. He conceives, too, that he is not yet "master of the best method of copying and enlarging the original pictures." It may confidently be expected that an experimenter of Mr. Hartnup's zeal and capacity, aided as he is, and will be, by the best chemical talent, will bring this art speedily to perfection; and that we shall soon have portraits of the moon painted by herself, and in which the drawing and effect are perfectly true. The last and most minute details must always probably be *directly* observed; but even an incompetent artist can fill up such details, when he has a good photographical sketch on a large scale before him. If any change is going on in our satellite, it may thus perhaps be made sensible in time.

At the same meeting of the British Association, the meteorological results of the Liverpool Observatory obtained the most flattering approbation. The Association has good reason to be proud of its creation, for the Liverpool Observatory sprung from the recommendation of that body in 1837, and few projects have been more successful. The anemometer results, diagrams, &c., were ordered to be printed entire in the next volume of the Proceedings of the Association.

The special object, however, of the Liverpool Observatory is that which was also the original object of the Royal Observatory,—to render navigation more safe. This humane and patriotic purpose has always been *first* in Mr. Hartnup's mind; and he has been furnished with the most ample means by his patrons. We have repeatedly attempted to draw attention to this most important subject, and Mr. Hartnup himself has taken every opportunity to press the matter on those most interested, but hitherto with only very partial effect. In chronometers of the usual construction, it is well known that the *compensation* for *heat* and *cold* is *imperfect*, but that the errors in a well-made time-keeper follow a certain fixed course, and can be tabulated for each instrument according to the indications of the thermometer. It has been proved by actual experience, that out of 100 chronometers, 95 will perform satisfactorily, where this special correction is applied. Now Mr. Hartnup supplies a table to every chronometer rated at the Liverpool Observatory, assigning the rate according to the tempe-

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rature in which the watch is going. The additional trouble is merely this. A thermometer of the rudest kind must lie near the time-keeper, and be noted once a-day, or, when the temperature is steady, once a-week. In bringing up the error, the seaman must use the daily rate *corresponding to the thermometer*, instead of *one* rate for the whole voyage. This operation would scarcely cost an additional minute a-week; and it would make every sound, well-made, but imperfectly compensated, chronometer, as good as a perfectly compensated chronometer, if such a thing ever exists. This method, however, is practised by but a small number of the English captains who sail from Liverpool, although they would have nothing additional to pay for the advantage of being rated in this manner at the Observatory. This fact, if it were not undeniably true, would surely seem incredible, that men should risk their own lives, those of their crew, and of their passengers, rather than take the trouble to use a rate corresponding to the daily temperature, instead of one rate for the whole voyage. Let us hope that the interest which has lately arisen with respect to the merchant service may be directed to this point.

With the consent of his Committee, Mr. Hartnup has been appointed an agent of the Board of Trade for the issue of instruments, logs, books, &c., at the Port of Liverpool. In matters of vital importance we cannot regret the appointment of so trustworthy a person, though astronomy may somewhat suffer. It must, however, be clear that in directing the attention of the Liverpool astronomer to so many objects, some of them must be neglected, unless due provision be made and assistance given. Sometimes, through thoughtlessness, more work is imposed on a willing man than human force can sustain; and we may venture to hint to the munificent patrons of the Liverpool Observatory that the limit, in Mr. Hartnup's case, has already been attained, and cannot be exceeded safely. Yet it would be a great pity to stop, or even to reduce, permanently, the astronomical work of this establishment, after so brilliant a career. The Observatory of Liverpool cannot well be spared, now that it has shown how useful it is and can be.

The Madras Observatory has put forth another volume during the past year, containing the results of observations from 1848 to 1852, while an Appendix brings up a certain portion of them to the beginning of 1854.

The most important part of the contents is a "Subsidiary Catalogue of 1440 Stars," being the result of a thorough revision of the British Association Catalogue between the limits of N.P.D. 40° and 155°, with notes, giving particulars of proper motion and other interesting points. This was a piece of heavy work requiring pretty constant labour for about four years, in observing and reducing. The agreement of the places with those given in the British Association Catalogue is, in the great majority of cases, pretty close, but there are about 100 whose errors are

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large, exceeding 1ˢ of time or 10″ of arc; some of these are mere blunders of whole minutes of time or arc, and a few may be cases of proper motion. Fifty-five numbers were missing, and the objects set down in the Catalogue as nebulæ appear, as far as they have been examined, to be loose clusters of small stars.

In the Planetary Observations, *Neptune* for the first time makes his appearance, and has been observed pretty assiduously since 1849; a few attempts were also made on some of the new planetoids, but they were speedily abandoned for want of sufficient optical power.

There is a pretty long list of double stars (or rather two lists, there being a second in the appendix), in which *α Centauri* holds a conspicuous place, having been carefully watched, and showing an angular motion of 30° in less than four years. These observations were made with the new equatoreal, by Lerebours and Secretan, of $6^{in.}\cdot 2$ aperture and $89^{in.}$ focus, which seems an effective instrument, though the object-glass originally furnished was of bad quality; but this defect has been remedied by the makers supplying a new one, which appears unexceptionable.

The observations in the appendix were all taken with this new glass; and we notice some pretty careful measures of *Saturn* and his rings, including the new features of the obscure ring, and fine dark line in the outer ring. There is also a second list of double stars, in which the most notable points are the indications of the binary nature of *α Piscium*, and of the parallax of *α Herculis*, which last is thoroughly discussed at the end, and shown to be $= 0''\cdot 060 \pm 0''\cdot 0041$, from the measures of position, a result nearly confirmed by those of distance; so that the quantity, small as it is, must be considered as *real*, until it can be disproved by observations equally numerous and accordant.

In April 1854, Captain Jacob was compelled to quit his post in search of health, and is now in England, his *locum tenens* being Major Worster of the Madras Artillery. The work now in progress there is an examination of those stars, especially Southern stars, affected (or supposed to be so) by proper motion to the extent of $0''\cdot 5$ annually; these amount to about 400.

The present state of the Observatory, as regards *matériel*, would seem susceptible of some improvement. The meridional instruments are rather behind the age. Originally small, and with some defects of construction, they have not been improved by somewhat rough usage for about a quarter of a century. The mural circle has large errors of division, by which most of Taylor's early observations are vitiated; and though these are now tabulated, and applied to every observation, this of course involves loss of time in reduction, besides being an eyesore. The transit instrument was seriously injured some years back in a hurricane, and though pretty well repaired is still not perfect; its optical power is too small to allow of its being used effectively on the new planetoids, a class of observation for which the geographical position of Madras is well suited.

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It is to be hoped that ere long the liberality of the Court of Directors will furnish this Observatory with a meridian circle similar, if not equal in size, to those lately erected at Greenwich and the Cape. Such an instrument would have been invaluable in the revision of the B.A. Catalogue above referred to; and there can be no doubt that it would materially add to the efficiency and usefulness of the Observatory.

At the last meeting of the Committee for restoring the standards of weight and length, the members were of opinion that a larger number of standards would be required than had been originally contemplated. Mr. Sheepshanks therefore applied to Mr. Simms for *all* the bars in his stock, which was readily complied with, Mr. Simms reserving only one for his own use. These additional bars have now been most carefully prepared, divided, and measured, so that the number of standard bronze yards amounts to forty. There are four more, which were cut down to 37 inches by Mr. Baily; but he left them undivided. These have been divided and compared. Altogether there are forty-four bronze yards and several more in other metals.

There is, and always must be, some uncertainty about the bisection of anything so imperfect as the best division is when highly magnified; and this uncertainty cannot be eliminated, as it varies, *accidentally*, with each division and each observer. But with the *same* observer a very satisfactory result is soon arrived at. When the divisions are fairly defined, 100 comparisons have a probable error varying from about a seven-millionth to a twelve-millionth of the whole, or somewhere about four millionths of an inch. This *degree* and *kind* of *certainty* has been arrived at in all cases. The bars, which, being observed in different manners, presented any anomaly, have been all re-measured.

Notwithstanding the elegance and security of Professor Miller's system (that of cutting the division at the bottom of a cylindrical well, which goes half-way through the bar), Mr. Sheepshanks is of opinion that it has considerable disadvantages. Even in Mr. W. Simms' hands, and with especial contrivances, it is rather uncommon to find both the gold pins, and both the divisions, perfectly satisfactory. The small aperture does not allow the surface to be worked to a uniform degree of *dead* brightness; the burr cannot always be well cleaned off without polishing or rounding the edges; and when in use, it is not easy to wipe off any occasional dirt which may interfere with the divisions. For bars which are to be floated in mercury the *well* is essential, but for ordinary purposes, Kater's plan of cutting away half the upper substance of the bar would be found more convenient. If, as Mr. Sheepshanks suspects, the *accuracy* of the measure depends *mainly* upon the sharpness and cleanness of the division, and the perfect uniformity of the face of the dot, every facility should be given to the workman to get at his work.

Besides the anomaly heretofore mentioned, viz. that practised

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and steady observers differ uniformly as to what *is* a bisection, Mr. Sheepshanks finds that the bisection greatly differs in his own case, according as the division crosses the eye vertically or obliquely. This difference is very large, with him, between the vertical and horizontal direction. Mr. W. Simms says that *he* does not find this to be the case, but he has heard it remarked by others. It will be odd enough if there is a sensible personal equation in *position* and *distance;* and it would be worth each observer's while to make out whether it is so or not, in his own case.

In making *end-yards* with *steel* tips no difficulty was found; but by an oversight, which Mr. Sheepshanks can scarcely explain, or excuse himself for, the bars hitherto made, tentatively, have all been too short. In other respects they were satisfactory. But even the comparisons that were made, though few in number, showed an evident *wearing* in the metal, and it seemed hopeless to *preserve* a standard touch-yard in use (though it is easy to get one) unless some harder material could be applied, and, if possible, one not corrosible. Professor Miller recommended quartz, and after one or two trials it was found very practicable to insert a conically-shaped piece of quartz into an iron or bronze socket, moderately heated. But when this was exposed to the cold of a freezing mixture, the quartz crashed: it was held firmly enough.

It was then considered that perhaps an intervening bed of soft metal would make the squeeze of the enveloping bronze or iron more uniform and more endurable; and Mr. James Simms further suggested that chalcedony would be found tougher than quartz. A request was made to Messrs. Elkington to deposit a layer of gold in a bed for the stone; but as this was declined by the firm in London, Mr. James Simms had the kindness to attempt to supply the want. Before, however, his attempts were perfectly successful, it was suggested that the chalcedony plug, which is very slightly conical, might be inserted in a heated gold or silver socket; that this again might be worked into a conical form and similarly inserted, in a reversed position, in the bar intended for a standard. A piece of chalcedony was thus fixed in a brass cylinder, which was afterwards turned very nicely and let into an iron socket, exactly representing the terminating portion of the end-bar. Exposure to a freezing mixture had no effect upon this combination, and the chalcedony was found to be of great toughness, such as would bear a considerable blow without injury. Some standards of end-measure would have been made before now, but the engineers have been so busy, that an order for steel bars has only just been executed. If any one can suggest a tougher or handsomer stone than chalcedony, and one which can be easily procured and of some size, Mr. Sheepshanks would feel greatly obliged by the information; but, so far as is known at present, this stone will do very well.

Besides comparisons of the discrepant bars and of the additional bars, the brass tubular scales which were measured in 1836 by Mr.

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Baily, have been compared with the new standard. The Royal Astronomical Society's standard, contrary to what was expected, was found to be nearly unaltered, that is to say, supposing the new standard to be exactly equal to that which is lost. But the other tubular scales presented considerable differences, viz., the Danish standard and those of Mr. Baily and Mr. Simms. All have been carefully determined, with a probable error considerably less than the hundred-thousandth of an inch.

The question now arises, how are these discordances in the tubular scales to be accounted for? And to say the truth it is not easy. Some further attempts have been made to ascertain the effect of the strain which undoubtedly exists in the apparatus used by Mr. Baily; but the results were not consistent. It seemed that the effect of the strain upon the microscopes was influenced by the greater or less friction which attends the motion of the frame, and which depends partly on the temperature. There are two evident objections to the tubular scales: they are too long, and they are very inadequately supported, at only two points. Mr. Baily's measuring apparatus is undergoing some alterations and improvements with a view to the subdivision of the yard, and the comparison of measures other than yards.

A yard with its Miller's stand has been prepared for the French Government, and will be conveyed to France in time to make its appearance at the Exposition.

The commission of the Government which, on Mr. Baily's death, was undertaken by Mr. Sheepshanks under the direction of the Astronomer Royal, is now brought to a conclusion, though there are several small matters to attend to which will take some time. But though we may hope that there never can be any doubt in future of the *length* of the *yard*, a great deal remains to be done to make the result of this long and expensive affair practically useful. For the business of ordinary life, standard-end bars and beds are required, and types, probably, of the units most frequently in use. It is very desirable, too, that accurate comparisons should be made between the English yard and the measures of those countries which possess an accurate standard. A well-divided scale of 40 inches is absolutely required for such researches and comparisons; and it would seem a necessary completion of the work, to connect the archetypes of the great national surveys with the new parliamentary standard yard. Whether any considerable portion of that which has just been described, can be effected by Mr. Sheepshanks is doubtful. The work hitherto has pressed rather heavily on his eyesight, if not on his health, and might now, probably, be advantageously transferred to younger and abler hands. But if the Government do not object to supply the necessary apparatus, and if he can satisfy himself that the work is competently executed, Mr. Sheepshanks is willing to continue his operations some steps further. He relies mainly on the advice and counsel of the Astronomer Royal, and on the good-will,

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intelligence, and beautiful workmanship of Mr. Simms and his house.

Within the past twelvemonth our knowledge of the group of small planets between *Mars* and *Jupiter* has been increased by the addition of six new members, which have been respectively named *Bellona, Amphitrite, Urania, Euphrosyne, Pomona,* and *Polyhymnia.*

Bellona was discovered by our Associate, Dr. Luther, director of the observatory at Bilk, near Dusseldorf, on the night of March 1. The period of revolution is 1700 days, or $4^{yrs}\cdot655$. The name was selected by Professor Encke.

On the same night, but about two hours later, Mr. Marth detected the planet which, on Mr. Bishop's suggestion, has been called *Amphitrite,* at the observatory of South Villa, Regent's Park. On the following night it was independently found by Mr. Pogson, who has been for several years attached to the Observatory of Oxford, where he devotes his leisure hours, after the regular duties of his office are completed, to the formation of charts of small stars, with the view to the detection of new planets or variable stars. A third independent discovery was made at the Imperial Observatory of Paris on the 3d of March by M. Chacornac, who is engaged on similar researches. The period of *Amphitrite* is 1484 days, or $4^{yrs}\cdot063$. The eccentricity of the orbit is less than that of *Ceres,* being the smallest in the group.

The *third* planet of 1854, called *Urania* on the proposition of Professor De Morgan, was found by Mr. Hind at Mr. Bishop's observatory on the night of July 22d. The time of revolution is about 1332 days, or $3^{yrs}\cdot647$, but has not yet been very satisfactorily determined.

Euphrosyne was found in a rather singular position on the 1st of September by Mr. Ferguson, of the Observatory, Washington, U.S. It was so close to the planet *Egeria,* of which Mr. Ferguson was in search, that it appears to have been observed along with it on that evening,—possibly from uncertainty as to which was the right object. Another night's observation proved that both were planets, the new one appearing of about the same degree of brightness as *Egeria.* The period of revolution is, probably, the same within a few days as that of *Hygeia* or *Themis.*

Pomona, so named by M. Le Verrier (who, as the Society will be aware, has succeeded to the direction of the observatory at Paris), was discovered by Mr. Hermann Goldschmidt, an amateur astronomer resident in that city, on the 26th of October. We already owed to the same gentleman the discovery of *Lutetia* in 1852. The approximate period is 1518 days.

Polyhymnia, the last of the six planets of 1854, also named by M. Le Verrier, was detected at the Paris Observatory by M. Chacornac on the night of the 28th of October. The elements are remarkable for the very large eccentricity they exhibit, whereby

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the difference between the perihelion and aphelion distances amounts to a diameter of the earth's orbit. The time of revolution appears to be about 1787 days, or $4^{yrs}.892$.

Since the last anniversary four new comets have made their appearance.

The *first* of these was for some time a very conspicuous object in the western sky. It was first seen in the south of France on the morning of March 24, the day before its perihelion passage. In this country it was remarked by several persons after sunset on March 28, and became generally visible on the following evening. It decreased rapidly in brightness, and disappeared after an interval of three weeks. A parabola satisfies the whole series of observations.

The *second* comet was discovered by Mr. Klinkerfues at Göttingen on the night of June 4, and three weeks later by Mr. Van Arsdale at Newark, in the United States. At the time of its perihelion passage it became visible to the naked eye. A sensible ellipticity was at first suspected, but it has not been confirmed. The whole series of observations, which extends to the end of July, cannot well be reconciled with a time of revolution of less than several thousand years.

The *third* comet had not less than six discoverers. It was found on September 11 by Mr. Klinkerfues at Göttingen; on the 12th, by Mr. Bruhns at Berlin; on the 13th, by Mr. Van Arsdale at Newark, N.J.; on the 18th, by Dr. Donati at Florence, and Miss Mitchell at Nantucket, U.S.; and, lastly, on September 21, by Mr. Gussew at Wilna. The observations, so far as they have been published, extend to the middle of November. The orbit does not appear to differ sensibly from a parabola.

The *fourth* comet, which is still visible, was found at the same hour, on the morning of January 15, by Mr. Winnecke at Berlin and M. Dien at Paris. It is faint, and very unfavourably situate for observations, so that they are not likely to be long continued. The perihelion has been some time passed.

The application of photography to astronomy is making sensible progress. Of Mr. Hartnup's proceedings we have already spoken. Mr. Phillips, at Oxford, has been engaged since the summer of 1853, and has furnished an account of his proceedings, which, it may be hoped, he will enlarge for insertion in the *Monthly Notices*. With an achromatic of $6\frac{1}{2}$-inch aperture, and 11-foot focal length, he first computed the *probable* place of the photographic focus, ·75 to ·80 of an inch beyond the focus. Trial proved that the photographic effect was evanescent within the focus, feeble at the focus, and greatest at about ·75 of an inch beyond the focus,—at least, for the moon's image.

In a letter to Col. Sabine, written in April last, Sir John Herschel strongly recommended that daily photographic representations of the sun should be made in some observatory, or

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rather in several, with a view to an historical record of the spots. The Kew Committee immediately entertained the suggestions of Sir John Herschel, and, after they had made some inquiries in regard of the probable cost of the necessary apparatus, came to the conclusion that the proposal should be submitted to the Royal Society, with a recommendation for its adoption. The Council of that Society ultimately decided that a photographic observatory should be erected in connexion with the Kew Observatory, and, with the concurrence of Mr. Oliveira, placed at the disposal of the Kew Committee the sum of 150*l.*, which that gentleman had liberally offered to the Royal Society, in aid of any scientific object which it might deem desirable to promote.

The Kew Committee having subsequently intrusted the carrying out of Sir John Herschel's views to Mr. De La Rue, one of its members, that gentleman, after deciding on a plan, engaged the services of Mr. Ross (well known for his success in the manufacture of telescopes and photographic lenses) for the construction of the telescope and stand, which are now progressing, and which, it is expected, will be erected in the course of three months.

The diameter of the object-glass will be 3·4 inches, and its focal length 50 inches; the image of the sun will be 0·465 inch, but the proposed eye-piece will, with a magnifying power of 25·8 times and focal length x, increase the image to 12 inches, the angle of the picture being about 13° 45′. The object-glass will be under-corrected in such a manner as to produce the best practical coincidence of the chemical and visual foci.* The eye-piece will consist of two nearly achromatic combinations, their forms, foci, and focal lengths, to be arranged upon the basis of the photographic portrait lens, the conditions being nearly similar.

It is contemplated to form the system of micrometer-wires on a curved surface; and it may ultimately be found to be advantageous also to curve the photographic screen, as the small curvature necessary, namely, about two-tenths of an inch, will present no mechanical difficulties. As in practice it may possibly be found desirable not to produce the sun's image with too great rapidity, a provision is contemplated for the absorption of some of the most energetic active rays by the interposition of coloured media of different tints.

The telescope being for a special object, it will have no appliances, except such as appertain exclusively to that object, so that the only means provided for *viewing* the sun will be through the finder intended for facilitating the adjustment of the sun's image in position, as regards the micrometer. The polar axis will be furnished with a worm-wheel and clock-work driver, and the declination axis with a clamping circle. A shutter for covering

* Mr. Ross has found that if for the greatest intensity of vision, in common lenses, the ratio of the dispersive powers of the two media is 0·65, that the chemical and visual foci will coincide but practically when with the same media the ratio is altered to 0·60; the media he uses being Pellatt's flint and Thames plate.

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the object-glass and capable of being rapidly moved by the observer, will be so contrived as to be under his command, whether he be, at the time, near the object-glass or near the screen, eight feet distant.

The telescope will be placed in an observatory, twelve feet in diameter, and provided with a revolving roof; adjoining the observatory, a small room for chemicals will be constructed, so as to facilitate the fixing of the pictures.

The attention of this Society has been directed with interest for several years to the labours of Professor Hansen, and especially to the two greatest works which he has undertaken,—namely, to the formation of new solar and lunar tables. The former of these works has been completed, and the solar tables have been published within the last year; and, as it is probable that many of our members are as yet but imperfectly acquainted with them, a few words on their construction may not be unacceptable.

Throughout this laborious work M. Hansen has been assisted by M. Olufsen, whose name appears on the title-page as one of the authors, and who has contributed elements (for example, the obliquity of the ecliptic) from his own researches.

The elements used in the construction of the tables are based on all the observations available for the purpose which have been made at Greenwich and Königsberg; and a table is added of comparisons of tabular places with the observations made at those observatories in the series of years from 1820 to 1843, which shows that the observations are completely represented within the limits of probable error by the tables.

M. Hansen proposes to give in a separate memoir the details of the calculations by which the various expressions explicitly given in the introduction have been formed, though the introduction, in a very lucid manner, explains the adaptation of these formulæ to the tabular arrangement.

One most important addition in these tables is the introduction of the right ascensions and declinations of the sun, as well as the latitudes and longitudes, for in the daily practice of astronomy it is these quantities which are needed most constantly, and in the formation of ephemerides the advantage derived from them will be found to be very great.

M. Hansen states the objects which he proposed to himself in the arrangement of the tables to be the following:—

1. That they should not only give the tropical apparent longitude, the logarithm of the radius vector, and the latitude of the sun, but also *immediately* the right ascension and declination, because at the present time the latter are of more frequent use in astronomy than the former.

2. To add the tables by which are obtained the mean right ascension, the equation of time, the reduction of the tropical ap-

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parent longitude to sidereal longitude, the diameter, the parallax, &c.

3. To construct the tables in such a way that they should give the quantities mentioned above with the same exactness as if they had been calculated immediately by the formulæ.

This great work is, undoubtedly, a valuable boon to modern astronomy; it is a work which has been long desired, and which undoubtedly combines the most remarkable combination of analytical skill and excellence of observations which have ever heretofore been brought to bear on any planetary tables whatever.

Since the last Annual Report was read Admiral Smyth has published his account of the Mediterranean, a work which, considered as an accompaniment to his charts by a nautical surveyor, is probably unique. As a manual of suggestions for the educated seaman, in his inquiries into the history, literature, remains of antiquity, commercial state, hydrography, meteorology, geology, and ichthyology of this most celebrated of all the waters on our globe, Admiral Smyth's work will at once take its place among the books of every traveller. The history of the charts of the Mediterranean will secure it a place in the hydrographer's library of research. To the astronomer it will have the specific value of giving power of ready reference to very many points of physics and geography, as they occur in connexion with the history and literature, as well as the application, of his science. And the scholar and the philologist will find the benefit of many elucidations of points of difficulty in his own pursuits. A general work on the Mediterranean can hardly fail to meet a want, now and then, in each and every branch of knowledge; and, in the present instance, no more of description than is necessary for sufficient announcement will justify the Council in congratulating the Society on the appearance of such a work under the name of one of our Fellows.

Among the publications of the past year which bear upon our branch of science the Council notice with much satisfaction a little work from the pen of Mr. James Breen, senior assistant at the Cambridge Observatory, entitled, *The Planetary Worlds.* One of our most excellent assistants, whose daily duties are sufficiently onerous to excuse the exertion, has taken advantage of his opportunity of access to a good astronomical library, and to one of the finest telescopes in Europe, and has found time to present the public with an interesting collection of descriptive matter, in a simple form, and of a kind frequently inquired for. The numerous woodcuts with which the volume is illustrated are hardly of a quality of execution to do justice to the letterpress, but, with a little correction by the reader's judgment, will be found materially to assist him in forming an idea of the various appearances seen by modern observers under bright optical aid.

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Professor Hansen has recently communicated to the President a detailed account of his progress in the construction of the Lunar Tables which for some time past have occupied so much of his attention. By a comprehensive discussion of the Greenwich Observations of the moon he deduced the corrections of the elements of the lunar orbit. A comparison instituted by him between the observations extending from 1824 to 1850, and the results of the theory thus improved, exhibits a most satisfactory accordance. The early observations of Bradley are also well represented by the theory, although, as might be expected, the agreement is not so close as in the case of the more modern observations. In the Tables which Professor Hansen is constructing from his theory the arguments are expressed, not in arcs of the circle, but in time, the unit of the argument being the mean time which elapses between two successive culminations of the moon. This unit is especially adapted to a comparison of the results of theory with a series of meridian observations of the moon; but when the question refers to the calculation of an ephemeris of the moon's place for fractions of a day, similar to that given in the *Nautical Almanac*, its advantages are not so obvious as Professor Hansen candidly admits.

The outstanding differences between observation and theory appearing to Professor Hansen to indicate the necessity of an enlargement of the coefficients of several of the most important inequalities in the moon's longitude, he was led to inquire into its origin, and he found that it might be to a great extent accounted for by supposing that the centre of gravity of the moon did not coincide with its centre of figure. This result is embodied in a very remarkable theorem, to which he has been conducted by his researches on the subject.

A total eclipse of the sun, although of extremely rare occurrence, is accompanied by phenomena of a highly important nature in regard to various questions connected with the physical constitution of the sun. An event of this kind is, therefore, always watched with unusual interest by the astronomer. On the 30th of November, 1853, there happened a total eclipse of the sun; but, unfortunately, it did not admit of being observed by the astronomers of Europe, the obscuration having been confined to the Pacific Ocean and a portion of the west coast of South America. The Chilian Republic, however, with praiseworthy zeal, despatched M. Moesta, the Director of the Observatory of Santiago, to a suitable station in Peru, near the central track of the moon's shadow; and a complete observation of the various phenomena of the eclipse was made by that astronomer, of which an account appeared in the *Monthly Notices* for June last. One of the features of this eclipse, which seems more especially worthy of notice, consisted in an interruption of the contour of the luminous ring usually seen around the dark body of the moon, arising from two apertures

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contiguous to the moon's limb, through which the dark ground of the heavens was visible.

After the death of Dr. Petersen, the publication of the *Astronomische Nachrichten* was carried on, provisionally, by Professor Hansen. Professor Peters has since been appointed to the Altona Observatory, which includes, in its understood duties, the editorship of the *Nachrichten*. Under his able management we may feel sure that this valuable journal will maintain its high reputation.

In the past year some difficulties have arisen in the transmission of the *Nachrichten* to English subscribers, *by post*, and there have been many vexatious stoppages and overcharges. An application from the President to the proper authority has at last put an end to these troublesome scruples of the subordinate officers, which were quite at variance with the liberal views of the heads of the establishment. We have to acknowledge, with thanks, that Mr. Rowland Hill has assisted us on every occasion to overcome the official fiction, and with great readiness.

The Fellows are aware that, according to our present system of publication, observations and ephemerides are for the most part turned over to the *Nachrichten*, as the best deposit for so much of practical astronomy, as does not appear in special publications. Amateur observers know this, and find the convenience of a means of communication which embraces the civilised world. Perhaps those friends of astronomy, who are not observers, may be told that there is scarcely any way in which they can encourage the science more effectually, than by contributing to a publication which is one of the main supports of astronomy, and a principal organ in its extension and improvement.

Papers read before the Society from February 1854 *to February* 1855.

1854.

Mar. 10. On a Mode of Mounting a Telescope Equatoreally. Mr. Rothwell.

Extracts from a Letter to the Astronomer Royal. M. O. Struve.

On the Physical Constitution of the Moon. Mr. Nasmyth.

Observations of a Comet. Mr. Drury.

On the Shadow of *Saturn* on the Ring. Mr. Hippisley.

Observations of Comet III. 1853. Mr. Heath.

Observations of Van Arsdale's Comet. Mr. Bond.

Discovery of a new Planet. Mr. Marth.

On Observing Spots on the Sun. Mr. Carrington.

Observations of Solar Spots. Capt. Shea.

Account of Hartwell Rectory Observatory. Rev. C. Lowndes.

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On two new Theorems relating to the Moon's Orbit. Rev. Prof. Challis.

April 12. On the Zodiacal Light. Rev. T. W. Webb.
Observations of *Amphitrite* and *Bellona*. Rev. Prof. Challis.
Occultations observed at Ashurst. Mr. Snow.
Observations of Comet I. 1854. Mr. Carrington.
Notice of a Comet. Mr. Lowe.
Elements of Comet I. 1854. Mr. Hind.
Occultation of *Mars* by the Moon. Mr. De La Rue.
Observations of Comet I. 1854. Mr. Burr.
Extract of Letter from M. Argelander.
Description of an Observatory. Mr. Dell.
Elements and Ephemeris of Comet II. 1854. Mr. Hind.
On the Comet of 1854. Dr. Forster.
Observations of the new Comet. Rev. Prof. Challis.

May 12. Occultations observed at Ashurst. Mr. Snow.
Elements of Comet II. 1854. Mr. Adams.
Note on Comet II. 1854. M. Littrow.
Note on Telescopic Observations. Mr. Lassell.
Observations of Comet II. 1854. Mr. Cooper.
Do. Do. Lieut. Goodenough.
Extract of Letter from Lord Rosse.
Extract of Letter from M. Laugier.
Observations of Comet II. 1854. Mr. Carrington.
Note respecting the *Astronomische Nachrichten*. The Astronomer Royal.
Elements of Comet II. 1854. Mr. Cooper.
Account of Comet, 1854. Dr. Rottenburg.

June 9. On the final Determination of the Longitude of Cambridge by Galvanic Signals. Rev. Prof. Challis.
Observations of *Amphitrite*. M. Argelander.
Occultation of *Mars*. Mr. Burr.
On a Method of finding the Longitude. Mr. De Boos.
Observations of Comet II. 1854. Rev. T. W. Webb.
Experiments in Lunar Photography. Mr. Hartnup.
Account of the Hartwell Observatory. Dr. Lee.

Nov. 10. Observations of Comparison Stars of Comet II. 1854. Rev. Prof. Challis.
Discovery of Comet III. 1854. M. Argelander.
Observations and Elements of Comet II. 1854. Mr. E. Powell.
Observations of Comet II. 1854. Capt. Jacob.
Ditto of Double Stars. Capt. Jacob.
On Elliptic Functions of the Third Order. Prof. Malmsteen.
Correction of the Elements of Comet II. 1854. Mr. E. Powell.
Numerical Values of Coefficients for the Perturbations of *Neptune*. Mr. Wackerbarth.

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On the Orbit of 70 *Ophiuchi*. Mr. E. Powell.
On the Resistance of an Ethereal Medium, &c. M. Angström.
Observations of Comet IV. 1854. Mr. Hind.
On the Difference of Longitude between Brussels and Greenwich, as ascertained by Galvanic Signals. The Astronomer Royal.
On a Method of finding Greenwich Mean Time at Sea. M. Rümker.
Extract of a Letter from Prof. Secchi.
Discovery of a new Planet. M. Goldschmidt.
Do. Do. M. Chacornac.
Account of Observatory and Observations of Satellite of *Neptune*. Mr. Lassell.
Notes on Chinese Astronomy. Mr. Williams.
On the Figure of the Moon. M. Hansen.
On the Construction of Lunar Tables. M. Hansen.
On the Eclipse of Nov. 30, 1853. Capt. Shea.
On a new Eye-piece for Solar Observations. Mr. Hodgson.

Dec. 8. On the Orbit of α *Centauri*. Mr. E. B. Powell.
Investigation of Foucault's Pendulum Experiments. Lieut. Ashe.
On Shepherd's Galvano-Magnetic Regulator. Mr. Ellis.
Telescopic Appearance of *Venus* at Inferior Conjunction. Dr. Drew.
On the Origin of the Attempts to deduce Invariable Standards of Measure from Physical Principles. Mr. Grant.
On the Satellites of *Neptune*. Mr. Hind.
On the Satellites and Mass of *Uranus*. Mr. Hind.
Note respecting the Pendulum Experiment. The Astronomer Royal.
On a Phenomenon relating to the Hill of Santa Lucia. Dr. Moesta.

1855.
Jan. 12. On Rating Chronometers by Lunars. Capt. Toynbee.
On the Orbit of ϵ *Coronæ Borealis*. Mr. E. B. Powell.
Observations of Zodiacal Light. Mr. Burr.
On the Telescopic Appearance of *Saturn*. Rev. W. R. Dawes.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Royal Society of London.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Asiatic Society.
Royal Institution.

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Royal Irish Academy.
Geological Society.
Linnean Society.
Society of Arts.
The Photographic Society.
Cambridge Philosophical Society.
The Philosophical Society, Liverpool.
The Philosophical Society, Manchester.
British Association.
University College, London.
Institute of Actuaries.
Corporation of Glasgow.
Hon. East India Company.
The Registrar-General.
The Superintendent of the Nautical Almanac.
The Radcliffe Trustees.
L'Académie National des Sciences de l'Institut de France.
L'Académie des Sciences de Dijon.
Société des Antiquaires de Picardie.
Royal Academy of Munich.
Royal Academy of Berlin.
Royal Academy of Brussels.
Royal Academy of Göttingen.
Royal Academy of Madrid.
The Society at Genoa.
The Academy of Science, Naples.
The American Philosophical Society.
American Academy of Arts and Sciences.
The Smithsonian Institution.
The Franklin Institute.
The National Observatory, Washington.
The Observatory at Brussels.
The Observatory at San Fernando.
The Editor of the Athenæum Journal.
The Editor of the Literary Gazette.

J. C. Adams, Esq.
G. B. Airy, Esq.
C. Babbage, Esq.
Prof. A. D. Bache.
Admiral Sir F. Beaufort.
Jas. Bedford, Esq., Ph. D.
W. Brown, Esq.
Sig. Carlini.
Rev. Prof. Challis.
M. Daussy.
Prof. De Morgan.
S. M. Drach, Esq.
Dr. Drew.
Prof. Encke.
M. Faye.
M. Gautier.
Dr. B. A. Gould.
R. Grant, Esq.
W. Gray, Esq.
M. Grunert.
J. Herapath, Esq.
Luke Howard, Esq.
Capt. Jacob.
Dr. Lee.
M. Liouville.
M. Von Littrow.
L'Abbé Moigno.
Don C. Moesta.

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Dr. Peters.
M. Plantamour.
W. Pole, Esq.
Prof. Quetelet.
Lieut. H. Raper.
W. Rathbone, Esq.
M. Reslhuber.
Capt. Shadwell.
Rev. R. Sheepshanks.
J. J. Sylvester, Esq.
Thos. Tate, Esq.
R. Taylor, Esq.
Sir W. C. Trevelyan.
M. Villarceau.
H. Warburton, Esq.
J. A. Welton, Esq.
T. T. Wilkinson, Esq.
Lord Wrottesley.
Mr. J. Williams.

Address delivered by the President, G. B. Airy, Esq. F.R.S., Astronomer Royal, on presenting the Medal of the Society to the Rev. William Rutter Dawes.

I have now to state to you, Gentlemen, that the Council have this year awarded the Medal of the Society to the Rev. William Rutter Dawes for his various astronomical works; and I take the opportunity of accompanying this statement with the following remarks.

A science so extensive as ours will necessarily present for the consideration of the Council, in the adjudication of the Medal, subjects of very different classes. We have sometimes to examine the difficult mathematical investigations of gravitational astronomy, and sometimes the less profound, though more bulky, calculations connected with the ordinary conduct of an observatory. We must sometimes balance the formal and dry labour of meridional observations against the less regular, but more interesting, work of extrameridional and micrometrical investigation. We must not estimate lightly the successful inventor of astronomical instruments; still less must we omit to recognise, in a science where everything depends on the connexion of past, present, and future, the important services of the literary astronomer. Finally, while we may sometimes hold ourselves bound to consider a single work of short duration, but of great merit, as demanding our best acknowledgment; in other cases we are equally required to extend our view over a long series of years, and to appreciate the duration as well as the specific excellence of an astronomer's labours. Thus it will usually happen that the subjects to which our Medals are adjudged, in the course of several successive years, have little similarity in their character or extent. The subject of the present medal differs in some respects from any which has been brought before you for a considerable time.

Mr. Dawes has been known for nearly a quarter of a century as a zealous private cultivator of astronomy, by extra-meridional and micrometrical observations. Distinguished as Mr. Dawes has been by an extraordinary acuteness of vision, and by a habitual,

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and (as I may say) contemplative precision in the use of his instruments, his observations have commanded a degree of respect which has not often been obtained by the productions of larger instruments. The first of his observations were made with a 5-feet telescope, of about 3 inches aperture; a few with a 7-feet Newtonian: subsequent observations have been made with Mr. Bishop's equatoreal and with a fine 6-inch Munich telescope. A part of these observations have been published in Mr. Bishop's volume, and a part in the *Memoirs* and *Monthly Notices* of this Society.

A large portion of the observations consists of measures of double stars. The first are two measures of ζ *Cancri* in 1831. These are followed by measures of 121 stars from 1831 to 1834, of 100 stars from 1834 to 1839, and of about 250 stars (in Mr. Bishop's volume) from 1839 to 1844. Of these stars, a great proportion are very close couples; in fact, Mr. Dawes seems to have been able successfully to grapple with nearly all the objects which have most severely tried the optical powers of the best foreign instruments. There are also various observations of occultations (one the occultation of a star by *Jupiter*); and I am unwilling to pass over the nearly singular observation (given to the world, I think, in another channel of communication) of *Jupiter* without any satellite visible.

To the astronomer who delights in the investigations based upon numerical measures, the observations to which I have referred will appear the most valuable. Yet, if I may venture a prediction, there are other observations by Mr. Dawes which will be cited more frequently: I allude to those on the physical appearances of the planet *Saturn*. First, the observations made in 1843 on the apparent division in *Saturn's* outer ring. Secondly, the phenomena observed in 1848, from June to December, at the disappearance of the ring. Thirdly, the independent discovery by Mr. Dawes, in 1850 November, of the dusky ring, and his subsequent observations on that wonderful appendage of *Saturn*. The last is, indeed, an admirable instance of what may be done with a comparatively small telescope, when it is used by an ardent intellect to sharpen the powers of an eye naturally acute. The astonishing phenomena of the total solar eclipse of 1851 were also observed by Mr. Dawes, who made the voyage to Sweden for that purpose.

I have thus far confined my notices to observations only, but I must not omit to remark, that astronomy is indebted to Mr. Dawes for several suggestions of extensive practical use. One of these is a series of proposals for determining a standard of optical power, and for laying down a scale of the magnitudes of stars. Another is a mode of observing the spots of the sun, which has given a more profound insight (I use the word in its geometrical and not in its metaphorical sense) into these remarkable craters than any preceding method.

I have recounted in detail these different astronomical works

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of Mr. Dawes, because it is on the assemblage of them that his claims to your honourable notice are based. Perhaps, if any one of them, or any one class of them, had been submitted to your Council, there might have been among the members of that body a disposition to examine carefully whether other single works might not be found presenting equal demands for their attention. But, in reviewing the assemblage of works—all unexceptionable in quality, of different kinds, continued through a long series of years—the Council have had no such misgiving. They feel that they have had ample grounds for recognising in the person of Mr. Dawes one of the best friends of astronomy, and in the works of Mr. Dawes, not only some of the most valuable contributions to astronomy, but also some of the most valuable patterns to future observers; and they are persuaded, that the approbation of the Society will accompany the Council's award of the Medal.

The President then delivering the Medal to Mr. Dawes, addressed him in the following terms:—

Mr. Dawes,—In the name of the Council of the Royal Astronomical Society I present you with the Medal of the Society, in acknowledgment of your long-continued devotion to astronomy, of your numerous contributions to various divisions of that science, of the remarkable excellence of your observations, and the recognised value of your suggestions. And I trust that there remain to you many years of health and happiness, many years of enjoyment of your favourite science, and many discoveries tending to the material and intellectual advance of astronomy.

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The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Manuel J. Johnson, Esq., Radcliffe Observer.

Vice-Presidents:

G. B. Airy, Esq. M.A. F.R.S. Astronomer Royal.
Augustus De Morgan, Esq.
John Lee, Esq. LL.D. F.R.S.
Admiral W. H. Smyth, K.S.F. D.C.L. F.R.S.

Treasurer:

George Bishop, Esq. F.R.S.

Secretaries:

Warren De La Rue, Esq. F.R.S.
Captain R. H. Manners, R.N.

Foreign Secretary:

John Russell Hind, Esq. Superintendent of the Nautical Almanac.

Council:

Arthur Kett Barclay, Esq. F.R.S.
Richard C. Carrington, Esq.
Rev. George Fisher, M.A. F.R.S.
James Glaisher, Esq. F.R.S.
Robert Grant, Esq. M.A.
Rev. Robert Main, M.A.
Rev. Baden Powell, M.A. F.R.S.
William Rutherford, Esq. LL.D.
Rev. Richard Sheepshanks, M.A. F.R.S.
William Simms, Esq. F.R.S.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *March* 9, 1855. No. 5.

M. J. JOHNSON, Esq. President, in the Chair.

J. B. Dancer, Esq., Manchester;
Wentworth Erck, Esq., 27 Herbert Place, Dublin; and
R. J. Mann, M.D., 7 St. Boniface Terrace, Ventnor,
were balloted for and duly elected Fellows of the Society.

Death of Professor Gàuss.

A letter was read from M. Hausmann, Secretary of the Royal Society of Sciences of Göttingen, announcing the death of this distinguished individual, on the 23d ultimo, in the seventy-eighth year of his age.

Account of Operations connected with the Advancement of Commercial Astronomy in Australia. By Robert L. J. Ellery, Esq.

(*Extracts of a Letter to the Astronomer Royal.*)

"Although not a member of the Astronomical Society, I take the liberty of communicating to you, as President of that body, the following information, deeming that the interest felt by the Society in the progress of science may prove a sufficient excuse for addressing them on so trivial a matter.

"In June 1853, a time-ball signal—visible to the shipping in Hobson's Bay—was erected on the mast or flagstaff, Gellibrand's Point. Soon after its erection, I was appointed to take the necessary observations for the regulation of this signal. At that time, two chronometers (neither of them first-class instruments), and two indifferent sextants, with a small artificial horizon, composed the staff of instruments. By using the system of equal altitudes on every possible opportunity, I ascertained the rate of one chronometer to be sufficiently reliable to enable me to give the time,

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even through a few days of cloudy weather; but the sudden and great changes of temperature act as a severe test to a chronometer's performance.

"In August, a small transit instrument was offered for sale, and bought by Government, with the works of a new regulator clock by Evans of Birmingham. Authority was given me to get a room built for the instrument and clock, which was done, attached to my quarters. The instrument was one of Spencer and Browning's 23-inch, on iron stand. Having some fine stone close at hand, I got some blocks cut, and a very substantial pier built. The building being of wood, the foundations for both transit-pier and clock had to be quite free from the building. The works of the clock were put in a case I got made here, and was mounted on a stone pier, built up about an inch above the level of the floor, with a large plank as a support at the back, built in with the foundation. The room had its meridional opening from horizon to horizon,—the south opening having a sea horizon, the north highlands at the back of Melbourne. After much trouble in adjusting the transit from the constant settling of the pier, from bad cement having been used, I got it into working order: got up a meridian mark, about half-a-mile away, consisting of three parallel vertical lines, finely marked with black paint on a white ground; and after that it soon became so steady as to only require occasional levelling. The clock, whose compensation was the mercurial one, after some little trouble, went very well and steadily in all the great extremes of temperature we are subject to (the temperature often has varied 45° from noon till midnight).

"With these instruments the time-signal has been given daily, Sundays excepted, up to February 1854, when a few instruments which had been ordered by the Colonial Government arrived, consisting of 30-inch transit by Potter (late Bates), regulator-clock, mercurial pendulum by Frodsham, sextant by Potter. These instruments were put into use at once; a new pier with a broader foundation was built for transit, and the old clock was removed to a room we use for chronometer-room, where a great many chronometers are brought for rating, &c. Up to this present time no other addition has been made, with the exception of one or two plain meteorological instruments. The clock by Frodsham is performing beautifully, having a steady rate of $-0^{s}\cdot3$ a-day, with scarcely any perceptible deviation for temperature. Several times I have noticed, when the barometer was at 30·20—30·25, a little more loss; but on the whole it has performed exceedingly well. The work that I have done with these instruments is as much as could be well done. The time-keeping is, of course, the main object at present; but, in addition, I am tabulating a regular list of zenith and circumpolar stars (of course of right ascension only, as the instrument has only a setting circle reading to minutes). I have, I think, determined the longitude of the observatory pretty closely, from the mean of

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35 sets of moon-culminations. Its previously assigned position was $9^h\ 39^m\ 41^s{\cdot}8$ E., 37° 52′ 42″ S.; but I make the longitude, $9^h\ 39^m\ 40^s{\cdot}0$ by the mean of 35 culminations. The assigned latitude I have always found correct. The previous position was, I believe, given by Captain Stokes, of one of her majesty's surveying vessels.

"The time-ball is a frame-work ball covered with canvass painted black, and is hoisted to the masthead by halyards, which halyards, when the ball is hoisted, are attached to a trigger below, and by a slight pull on which the halyards are released the ball drops. It is rather a primitive arrangement, but answers pretty well. Since the chief station of the electric telegraph has been completed at Melbourne, a time-ball has been placed on the tower of that building for the benefit of the city of Melbourne, and it is dropped at the same instant as the one here by means of the telegraphic wire. As yet no coil magnets are used for releasing the detents, as we have not been able to get them made yet; but I am only now waiting for authority to get the connexion as complete as possible. As it is now, I have to take a previously compared chronometer to the Flagstaff, a distance of about 50 yards from the observatory, and by the side of the trigger of the time-ball is a little brass *circuit closer* (the whole line being in complete circuit from 5 minutes to 1 o'clock, except at the Flagstaff, where the signal is given from). The ball in Melbourne being hoisted at the same time it is here—3 minutes before one—I connect the circuit several times to see all is right; each connexion is made known to the telegraph operator in Melbourne by the deflexion of a small needle indicator; he then stands in readiness with his hands on the trigger, and 5 seconds before one—I give seconds beats of the indicator—by connecting each second at the fifth beat both balls are released; and, from repeated watching, no perceptible interval in their dropping is perceived. The time-balls are about 8 miles apart: the one in Melbourne is a little to the eastward of the observatory, —about 2 seconds' time, I think. The exact instant of the drop of the Williamstown time-ball is marked by my only assistant, my wife; and if any error occurs, from wind or otherwise, it is thus accurately accounted for, and published in the daily papers.

"It is proposed to erect time-balls on the electric telegraph stations, which are now in course of erection at Geelong and Port Phillip Heads, so as to make every use of the observatory possible in giving mean time to different parts of the colony. The plan for these time-balls I have given myself, and will be constructed so as to be dropped by direct galvanic current without any manual intervention; and I wish to adopt the same means that you have done with regard to the time-ball at Deal (I think it is Deal),—viz. an apprisal by the drop of the signals themselves that they have dropped correctly. In addition to time-balls here, since June last another means has been adopted as a time-signal,—viz. the light of the lighthouse on Gellibrand's Point. It having been

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represented to the chief harbour-master that on account of the great crowd of shipping those lying towards the opposite shores could not see the time-ball, and begging that some means might be adopted to make the signal more generally seen, the harbour-master had an apparatus fitted to the inside of the lantern, which obscures the light by releasing one trigger, and shows it again suddenly by releasing another. This was his own arrangement, and my instructions were to obscure the light at 2 minutes to 8 P.M., and suddenly show it at 8 o'clock, the latter being the true signal, any error of which would be published in the daily papers. It answers very well, but is very inconvenient, climbing the lighthouse-tower with a chronometer in hand. The master-mariners think it a great convenience, as it allows them to attend to the rating of their chronometers themselves, for business often keeps them on shore during the day. If this is continued, I must get some better plan devised: the telegraph wires could be used here too.

"The sum of 2500*l.* was voted by the Legislative Council for the building a stone observatory at Williamstown; this has not been done yet, but the Governor has placed in the estimates for next year the sum of 660*l.* for the purchase of astronomical and meteorological instruments, according to a list of what would absolutely be required that I was instructed to send in. The chief of these instruments were,—a transit circle, by Troughton and Simms; a 5 or 6-feet equatoreal by Troughton and Simms, or Ross; a portable transit collimator, &c.; with a small equipment of good meteorological instruments. In the ultimate purchase of these instruments, perhaps you might give some advice, from your great experience in such matters. At all events, I shall take the liberty of informing you when the orders are sent home for these instruments, in case you might feel any interest in guiding the purchase of them. I had omitted to say, that since December last I have kept a regular journal of three-hourly meteorological observations,—at least, so far as my limited staff of instruments allowed me, having only a simple, but very good pediment barometer, Zambra's thermometer, and aneroid barometer. Having neither rain-guage, anemometer, or hygrometer, of course my observations are very limited; but, such as they are, they represent some interesting facts with regard to Australian meteorology.

"*Observatory, Gellibrand's Point,*
Williamstown, Victoria."

Account of the Steps recently taken by Her Majesty's Government for Promoting the Regular Observation of Meteorological Phenomena at Sea. By Capt. Robert Fitzroy, R.N.

"The importance of accumulating meteorological observations,

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and tabulating them methodically, for the purpose of future, rather than immediate investigation, having been urged by the Royal Society, while the practical benefits arising from such collections, even at the present time, were proved by the direct consequences of Maury's extensive labours, Her Majesty's Ministers agreed to establish an office under the Board of Trade for receiving and tabulating all such observations made at sea.

"It was considered that much information might be compiled with respect to currents, as well as winds, which might be made more generally known to those interested in the passages of ships across the ocean; and that the sooner such authentic compilations could be made generally available, the greater would be their value. It was, moreover, pronounced to be necessary that instruments of a reliable and understood nature should be alone employed; that they should be carefully tested and vigilantly guarded from accidental causes of error.

"To meet these objects, an estimate of probable expenses was submitted to Parliament, and the sums proposed were voted, namely, 2000*l.* for the Mercantile Marine and 1000*l.* for her Majesty's ships.

"Soon afterwards an officer was appointed to execute the duties of the Meteorological Office, to be subsequently assisted by a few subordinates; but some time elapsed before instruments of the peculiar kind deemed proper by a Committee of the Royal Society could be finished, and an office appropriated for the object in view. Now the preliminary arrangements are made, and the Meteorological Office of the Board of Trade is open at No. 2 Parliament Street.

"A certain number of selected ships of the Mercantile Marine, and all those of her Majesty employed in long or distant voyages, are, or soon will be, engaged in making exact observations with instruments supplied under the authority of the Board of Trade (duly tested and compared), and in registering the apparent results according to forms settled at the Brussels Conference of 1853, slightly modified, however, so as to suit present convenience.

"The estimates sanctioned by Parliament are sufficient to provide sixty merchant-ships and forty men-of-war with the necessary meteorological instruments (namely, barometers, thermometers, and hydrometers), in addition to the nautical instruments usual at sea; to pay office expenses and salaries (including allowances to agents at outports); and to provide the necessary registers. A captain in the navy is in charge of the office. Four subordinates are to assist him, and there are agents appointed at the principal ports to communicate personally with the owners, captains, and officers of ships.

"Liberally supplied by the United States Government, Maury's Sailing Directions and Charts are distributed gratis among those who undertake to record observations satisfactorily, and send them to the Board of Trade. Marks, expressive of distinction, are to be annexed to the names of approved contributors to meteorology

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in the Mercantile Navy List, and other encouragements are contemplated.

"Every exertion will be made at the office, not only to discuss and tabulate valuable observations, but to digest and render available, as soon as possible, such information as may tend immediately to the improvement of navigation.

"*February* 8, 1855."

On the Application of Photography to Astronomical Observations.

(*Letter from Sir John F. W. Herschel to Colonel Sabine.*)

"I consider it an object of very considerable importance to secure at some observatory, and indeed at more than one, in different localities, daily photographic representations of the sun, with a view to keep up a consecutive and perfectly faithful record of the history of the spots. So far as regards the general delineation of the whole disk, and the marking out on it, in reference to the parallel to the equinoctial passing through its centre, the places, sizes, and forms of the spots, there would need, I should imagine, no very powerful telescope,—quite the contrary; but it should be equatoreally mounted, and ought to have a clock motion in the parallel. The image to be impressed on the paper (or collodionized glass) should be formed not in the focus of the object-lens, but in that of the eye-lens, drawn out somewhat beyond the proper situation for distinct vision (and always to the same invariable distance to insure an equally magnified image on each day). By this arrangement, a considerably magnified image of the sun, *and also of any system of wires* in the focus of the object-glass, may be thrown upon the 'focussing-glass' of a camera-box adjusted to the eye-end of a telescope. By employing a system of spider-lines, parallel and perpendicular to the diurnal motion, and so disposed as to divide the field of vision into squares, say of 5′ in the side, the central one crossing the sun's centre (or rather as liable to no uncertainty, one of them being a tangent to its lower or upper limb), the place of each spot on the surface is, *ipso facto*, mapped down in reference to the parallel and declination circle, and its distance from the border, and its size measurable on a fixed scale.

"If large spots are to be photographed specially with a view to the delineation of their forms and changes, a pretty large object-glass will be required, and the whole affair will become a matter of much greater nicety; but for reading the daily history of the sun, I should imagine a 3-inch object-glass would be ample.

"The representations should, if possible, be taken daily, and the time carefully noted. As far as possible, they should be taken at the same hour each day; but in this climate, a clear interval, occurring when it may, had better be secured early in the day.

"Three or four observations in tropical climates, distant se-

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veral hours in longitude (suppose 3, at 8^h distance in longitude), each recording at, or nearly at noon, would, when the results were assembled, keep up a continuous history of the solar disk.

"With regard to proper preparation of paper, or the use of collodion acid, the photographic art is now so much advanced, that no difficulty can arise in fixing upon fitting preparations, or the manipulations necessary for multiplying them. But it would be very requisite that many impressions of each day's work should be taken and distributed, and an interchange kept up among observers.

"*April* 24, 1854."

Observations of Comet I. 1855. By Dr. Donati.

(*Communicated by Mr. Drach.*)

	Florence M.T.	Comet—Star. Δα.	Δδ.	Comet's Apparent R.A.	Decl.
1855.	h m s	m s	′ ″	h m s	° ′ ″
Feb. 15	17 9 8·1	+4 7·02	−17 34·0	16 30 57·37	−28 12 31·8*
	17 37 38·5	−4 55·76	+ 1 57·7	16 30 59·30	−28 12 30·3†

* 5 comparisons with *a*. † 3 comparisons with *b*.

Adopted Apparent Places of Comparison Stars.

		R.A.	Decl.		
		h m s	° ′ ″		
a	Feb. 15	16 26 50·35	−27 54 57·8	Piazzi 113	Hora xvi.
b	—	18 35 55·06	−28 14 28·0	— 159	—

The comet was very faint.

On the Constitution of the Atmosphere, upon which Laplace's Table of Astronomical Refractions is founded. By Sir J. W. Lubbock, Bart. F.R.S.

"I have elsewhere given a table showing the constitution of the atmosphere, upon which Ivory's table of refractions, and also that upon which my own table of refractions, is founded.

"I have calculated the following table from the expressions given by Laplace in the fourth volume of the *Méc. Cél.* p. 265:—

Height in Miles.	Laplace. Pressure, inch.	Temp. Fahr.	Density.
0	29·94	32·00	1·00000
1	24·33	6·92	·85744
2	19·56	−14·14	·72283
3	15·57	32·60	·60010
4	12·31	47·24	·49114
5	9·60	60·23	·34709
10	2·54	111·21	·11743
15	·60	120·00	·02937
20	·13	139·70	·00674

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"The pressures in Laplace's theory are up to the height of five miles *less* than those of Ivory or my own, so that in calculating heights by means of the barometer, Laplace's formula would give the heights rather greater; the difference would be, roughly,

At a height of	1	mile,	263 feet
—	2	.....	519
—	3	.....	749
—	4	.....	898

"The pressures in my atmosphere and that of Ivory within these limits may be considered as identical.

"The temperatures in Laplace's atmosphere diminish far more rapidly at first than in Ivory's atmosphere or mine, namely, at the rate of a degree of Fahrenheit for every 210 feet: in my atmosphere and in Ivory's they diminish at the rate of about 1° Fahr. for 350 feet.

"The density in Laplace's atmosphere is greater than in Ivory's or mine.

"The following table, which has been published elsewhere, is repeated here for the sake of comparison:—

Height in Miles.	Lubbock. Pressure. inch.	Temp. Fahr.	Density.	Ivory. Pressure. inch.	Temp. Fahr.	Density.
0	30·00	+ 50·0	1·00000	30·00	+ 50·0	1·00000
1	24·61	35·0	·84611	24·61	34·4	·84875
2	20·07	19·5	·71294	20·05	18·3	·71373
3	16·25	+ 3·4	·59798	16·22	+ 4·9	·59472
4	13·06	− 13·3	·49903	13·07	− 6·3	·49118
5	10·41	30·6	·41403	10·47	16·2	·40230
10	2·81	126·4	·14499	3·25	45·8	·13407
15	·45	240·6	·03573	·95	56·2	·04044
20	...	...	...	·28	59·4	·01175
22·35	...	−448·0	·00000			
30	...	...	...	·02	60·6	·00097

"It appears to me that accurate observations are still wanting to determine the rate at which the temperature diminishes with the altitude, and especially to decide whether the rate be greater at night than by day. This is the more important as astronomical observations are chiefly made at night. Where I reside in the country, the situation is somewhat elevated, and the climate is cold in consequence, as I believe, but the nights are much colder in proportion than the days."

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Remarks on Mr. Powell's Elements of the Orbit of 70 Ophiuchi.

By Isaac Fletcher, Esq.

"The *Monthly Notices* for December last contain an interesting and very able discussion by Mr. Powell of the orbit of this remarkable star. So far as the agreement between the observed and computed angles of position is concerned, Mr. Powell's orbit represents, with singular fidelity, the actual path of the companion star. As Mr. Powell remarks, the agreement is less satisfactory as regards the distances, and it is on this point that I am desirous of making a few brief remarks.

"If Mr. Powell's orbit is correct, the distance of the two stars is still increasing; and Mr. Powell is of opinion that the measures of distance in existence are not sufficiently trustworthy to warrant an alteration in the elements sufficient to reconcile the great discrepancy between the calculated and observed distance for the epoch of 1854. This discrepancy amounts to 0″·538.

"I can hardly believe that an error of such magnitude can exist in the measured distance of so easy a star as 70 *Ophiuchi*, especially if that distance is the result of several nights' observations by so experienced an observer as Capt. Jacob. Having carefully examined all the observed distances of 70 *Ophiuchi*, I find the individual results of each observer, as well as the united testimony of all, concur in indicating an *increase* in distance up to the epoch of 1849 or 1850, since which time it appears to me there is evidence equally unequivocal of a *decrease* in that element. If this be really the fact, I think, as Mr. Powell has pointed out, a slight alteration in the position of the node, or of the periastre, would reconcile the theoretical orbit with the actual fact. As I have a pretty strong opinion that the distance is now diminishing, I beg to submit a statement of all the measures of distance I am acquainted with since 1850.

By Capt. Jacob.

Epoch	1850·571	Distance	6″·86
—	1852·75	—	6·73
—	1854·081	—	6·365

"The above are taken from Mr. Powell's paper.

"In the last supplement to the *Notices*, however, Capt. Jacob's result for 1854 is thus stated:

Epoch 1854·262 Distance 6″·51

Whichever value, however, is adopted, my agreement is not weakened, viz., that Capt. Jacob's measures show a decided decrease in distance.

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By Mr. J. F. Miller.

Epoch	1851·64	Distance	6″·508
—	1852·71	—	6·466

The first of these results is derived from a mean of 32 measures, and the latter from a mean of 22.

By myself.

Epoch	1850·66	Distance	6″·459
—	1851·58	—	6·378
—	1852·63	—	6·362
—	1854·62	—	6·300

The first result is from a mean of 56 measures, the second from a mean of 126, and the third from a mean of 78, but the fourth is merely a single night's result, and therefore of little value.

"An inspection of the above measures shows that three different observers, working independently, have all obtained results which conspire to prove that the distance is now decreasing.

"If the distance is really increasing, it is, at any rate, extraordinary, that these results should indicate the contrary.

"In making these remarks, I am very far from wishing to cavil at Mr. Powell's elements. On the contrary, I think that his orbit is decidedly the best that has yet appeared, but I think this able computer has hardly given to the measures of distance the weight to which they are entitled. If the elements could be varied so as to represent the recent observed distances with tolerable precision, without introducing more serious differences in the angles, it would, I think, be more satisfactory.

"Mr. Powell's important researches have, however, indisputably established a fact which has been doubted in high quarters,—that the relative motions of the components of 70 *Ophiuchi do* follow the Newtonian laws of gravitation.

"*Tarn Bank, Jan.* 24, 1855."

On a Telescopic Appearance seen in the Moon.

By Robert Hart, Esq.

(*Extract of a Letter to the Astronomer Royal.*)

"Since I wrote you last [respecting the two fiery-looking spots in the moon's disk] I have taken every opportunity of examining the same region of the moon while it was in shadow, and also during the early part of this moon, and most carefully when she was of the same age. But there is no such appearance of any such light as was so visible on the night of the 27th December last, between 7 and 12 P.M. (the time that I observed her).

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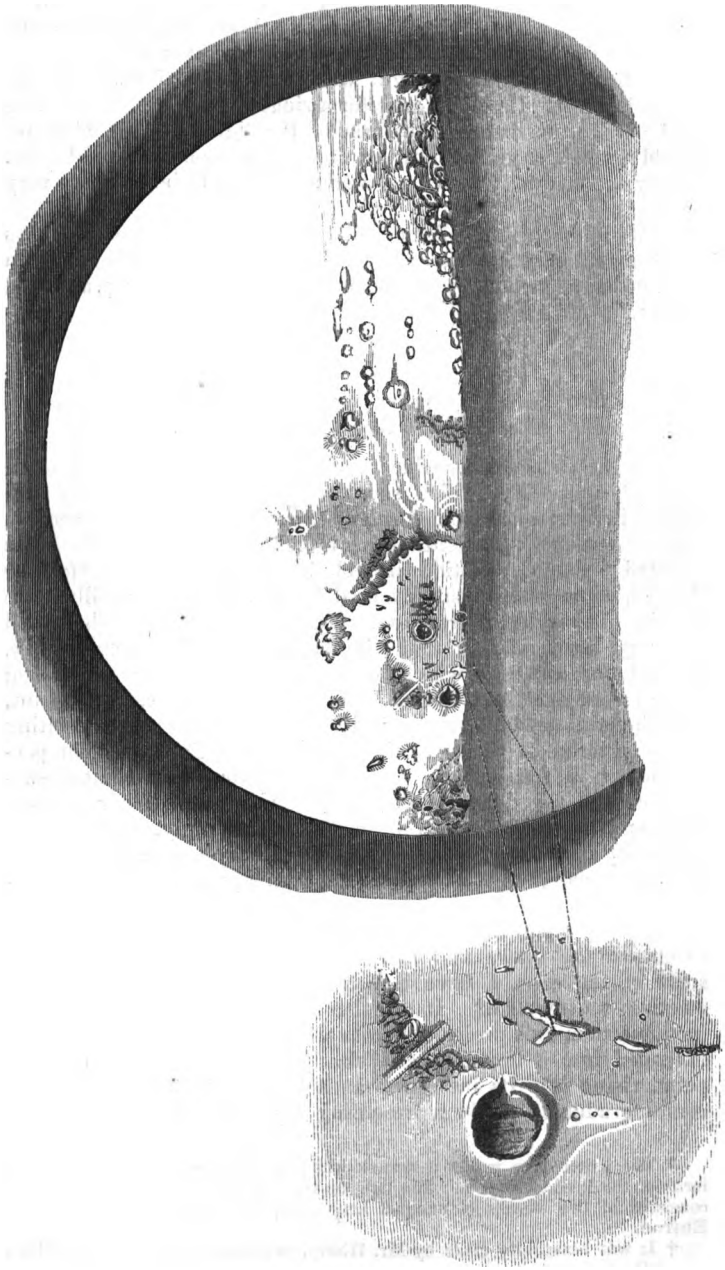

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"I have made a sketch, which I enclose, of the region where they were; and again when she was two days older, and when the ridge was full in the light I made the enlarged sketch.

"I have no doubt the light was from the two small mouths shown in the angles of the cruciform ridge.* . . . In looking at the ridge, as drawn, the form of the lights is just what we should expect anything flowing from these mouths would take. If they are mouths, which I am assuming them to be, they are very small.

"My telescope is a reflector of 12 feet focal length and 10 inches in diameter, of our own making. The speculum has not been polished these twelve years, and may not show so sharp as it did at first."

Description of an Apparatus for the Mechanical Imitation of Precession. By T. W. Burr, Esq.

"Being desirous, for educational purposes, of obtaining some piece of mechanism which should represent the motion of precession in a more tangible form than can be done by diagrams on a plane surface, my attention, while considering the subject, was arrested by a description in vol. i. page 43, of the *Monthly Notices*, of an instrument invented by Mr. Atkinson, to illustrate certain phenomena of rotation. The account is very short and not very lucid, stating that 'it consists of a flat circular disk, through the centre of which passes a small endless screw, having a cup in one of its extremities.' When this cup is set on a point, and the disk made to rotate in its own plane, the whole rotating plane performs at the same time a slow revolution round the perpendicular to the horizon which passes through the point of suspension, and the direction of this revolution will either coincide with, or be the reverse of, the rotation, accordingly, as the centre of gravity is above or below the point of suspension; affording in the latter case, says Mr. Atkinson, 'an apt illustration of precession.'

"Acting upon this suggestion, and adding such improvements as enable the instrument more effectually to represent the real state of the case, I have had the apparatus submitted to the meeting constructed.† It consists of a small terrestrial globe, having a steel axis projecting from the north pole, and the equator extended into a broad brass disk. The globe is suspended on a point, rising into its interior from a base which supports a horizontal graduated circle representing the ecliptic; and the whole

* The writer here makes a remark which is not very clear respecting the form of the luminous spots. The light on the right side would appear to have consisted of two streaks of unequal length disposed in opposite directions.—Editor.

† It was beautifully made by Mr. Hislop, chronometer-maker, 108 St. John Street Road.

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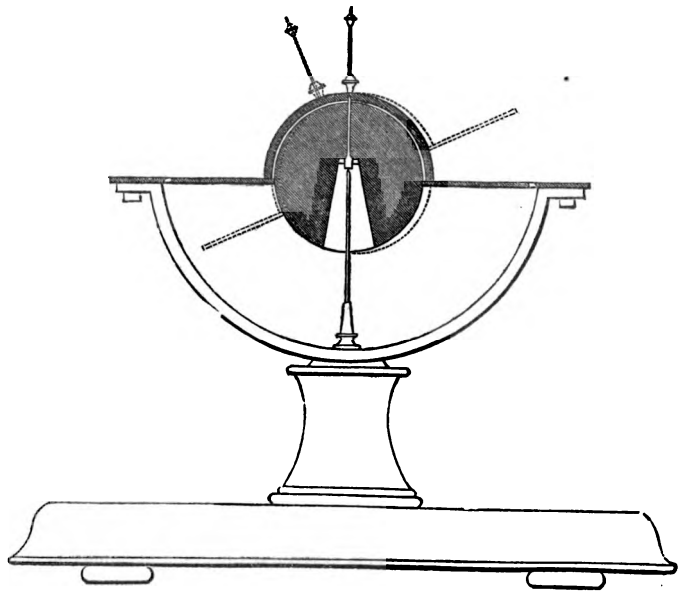

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being nicely balanced, the ecliptic and equator coincide. If now a small weight be fixed to the equatoreal disk, to represent the attraction of the sun and moon, or the axis be considerably inclined, which answers the same purpose, and the globe be rapidly rotated, representing the diurnal motion, it will be seen that at the same time there is generated a slow conical movement of the projecting axis round the imaginary pole of the ecliptic, in a direction opposite to that of the diurnal rotation; and that the equinoctial points, or intersections of the ecliptic and equator, move round in the same way."

The author remarks that, upon attentive observation, it will be found that the axis does not describe a perfect circle round the perpendicular to the ecliptic, but that there is a minute undulation resembling the nutation of the earth's axis.

"I am aware that the mechanical causes of the motions set up are not the same with the physical causes of the real motions of the earth, but the object was to obtain an imitation of the effect, regardless of the exact identity of mechanical causes. So far as I understand the description of Mr. Atkinson's instrument, there is, of course, an identity of principle and effect, but he seems to have had other points to illustrate, and only mentions precession as an incidental one; whereas, in my construction of the apparatus, those additions are made which enable it to carry out the imitation to the fullest extent; and, having myself found it useful in explaining the motions referred to, I have thought a knowledge of it might be acceptable for a like purpose to other members of the Society. The drawings show the apparatus in perspective and section, and are sufficient, with the preceding description, to explain it without references."*

Note by the Editor.

In the *Edinburgh New Philosophical Journal* for April 1855 there is a paper by Mr. James Elliot, Teacher of Mathematics, Edinburgh, which also contains a description of an apparatus

* Apparatuses for exhibiting the conical movement of the earth's axis, which occasions the precession of the equinoxes, have also been contrived by Bohnenberger, Fessel, and Professor Powell. They are all unexceptionable, both in principle and practice, and serve to represent the phenomenon with the most complete fidelity. This remark will appear superfluous to many persons, but it has been deemed necessary to advert to the circumstance in consequence of objections having been urged against those apparatuses, which could only arise from an imperfect knowledge of the mechanical principles of their construction. Nor can such objections be justified by referring them to an imperfect working of the apparatus in any case, since from the nature of the mechanism it is impossible that the axis of rotation, if it have a movement at all, should have any other than a conical movement. In all these contrivances (as in Atkinson's original apparatus, now in the possession of Mr. Riddle at Greenwich) there is an arrangement of great simplicity, by which the conical motion of the axis of rotation may be rendered *direct*, as exemplified by the spinning-top or *retrograde*, as in the case of the earth's motion, thus affording a most instructive view of the mechanical principles which determine the grand phenomenon of the precession of the equinoxes.—Editor.

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exactly the same as Atkinson's; but the author does not seem to be aware of the fact that any person had preceded him in the invention. The paper, which is entitled "A Description of certain Mechanical Illustrations of the Planetary Motion, accompanied by Theoretical Investigations relating to them, and in particular a new Explanation of the Stability of Equilibrium of *Saturn's* Rings," was read before the Royal Scottish Society of Arts on the 27th of February and 13th of March, 1854.* It contains several ingenious illustrations of the planetary movements in addition to that of precession. The author states that he was first led to consider the possibility of imitating the conical motion of the earth's axis by observing the whirling of a penny, and that afterwards, from reading Herschel's *Astronomy*, his attention was turned to the analogous movements of the te-to-tum and the top. "I observed, however," says he, "that in tops which have short pegs, this motion—the conical motion of the axis—is slower than in those which have long ones; and, in fact, the shorter the peg the slower the revolution. It therefore occurred to me, that if we could lower the centre of gravity till it coincided with the centre of motion, this movement would cease altogether, and the top would continue to spin with its axis pointing permanently in any direction in which it might be placed. I also concluded, if we still further extended the same change which gradually annihilated the positive motion, it would reappear negative or in the opposite direction. With that view I had an instrument constructed of the form showed in the annexed cut,† consisting of a wooden ball hollowed out in its lower part, so as to admit the support upon which it rests to be raised above the centre of gravity, and with a screw upon its peg or axis, to admit of its being raised or lowered at pleasure. I also confined it to one place, by forming a small cavity on the support for the point of the peg to run in. This being done, I was much pleased to find my expectations exactly realised. By adjustments of the screw the conical revolution could be quickened, retarded, annihilated, or reversed, as might be desired, and all its motions were brought under perfect control. At the same time it was surrounded by a fixed plane to represent the ecliptic, its own equator being marked upon it; and by forming the axis of hard steel and giving it a support of agate, its velocity could be kept up without much abatement for a long time.‡

"The rotation is produced in the ball by means of a string and handle, much in the same way as that in which a humming-top is spun. The case in which, from the two centres coinciding, the axis remains fixed in one direction without any conical revolution,

* It is stated that the silver medal of the Society, value ten sovereigns, was awarded to the author on account of his paper.

† The drawing here alluded to is almost identical with that annexed to the preceding paper.

‡ The author here states in a note, that since the model described was constructed, his attention had been directed to Bohnenberger's instrument, of which he was not previously aware.

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enables us to illustrate clearly what is meant in astronomy by the parallelism of the earth's axis, since the model may be carried by the hand slowly round in any circular or elliptic orbit, without any perceptible deviation of the axis from its original direction. But when the centre of gravity is brought slightly below the point of support, we are then enabled to show the deviation from parallelism which arises in the direction of the earth's axis after a long period of years, the same motion exhibiting the precession of the equinoxes. With the centre of gravity so placed, if the ball is made to rotate in the direction marked by the upper arrow on the figure, or from west to east, the equinoctial point E is observed to move slowly in the direction marked by the lower arrow from east to west. The latter motion may be made as slow as we please, so as to approach within any degree of closeness the exceedingly slow precessional movement of the earth's equator."

In another part of his paper the author makes the following remark:—

"If we next load the sphere on one side *very slightly* by any means, we obtain an illustration of the nutation of the earth's axis, the axis making a multitude of minute conical revolutions round the circumference of the greater conical revolution."

Note on the Method of Computing the Moon's Parallax. By Mr. Sang.

In deducing the true from the apparent place of a heavenly body, it is usual to correct the observed altitude for the effect of atmospheric refraction, and to use the altitude thus corrected as the argument for the parallax; the sine of the parallax being supposed to be proportional to the sine of the corrected zenith distance.

This operation is founded on the hypothesis, that on correcting the observed altitude for refraction we obtain that altitude at which the planet would have been seen from the observatory if there had been no atmosphere. This hypothesis, however, is incorrect, as is evident when we consider that the ray of light, impinging on the upper surface of the atmosphere, is gradually bent downwards, and reaches the eye after having traced a curved path through the air. The angle of refraction is that angle which the first direction of the light makes with its direction when just entering the telescope; and, therefore, on correcting the observed altitude for refraction we obtain the direction, not of the original unrefracted ray, but of a line drawn parallel to it through the place of observation, which line cannot pass truly through the planet.

Thus if A be the observatory on the earth's surface, M the position of a celestial body, and M B the ray of light entering the atmosphere at B and then describing the curved line B A, the

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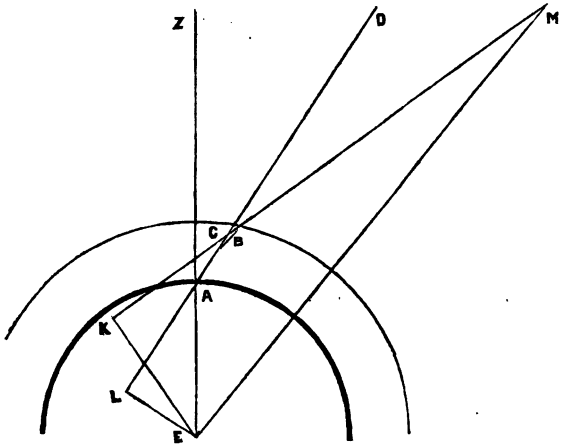
line A D drawn to touch that curve at A gives the apparent position of the planet; and if we continue M B to meet the tangent in C, M C D is the angle of refraction. When, therefore, we add the refraction D C M to the observed zenith distance Z A D we obtain the zenith distance of a line drawn through A parallel to C M; and this corrected zenith distance exceeds that which the planet would have had if there had been no atmosphere by the angle A M B, under which the curved path B A would be seen from the planet.

In the case of the primary planets this angle A M B is much too small to be taken into consideration, but in the case of the earth's satellite it may possibly amount to something appreciable. My object in the present paper is to estimate the amount of error caused by neglecting this angle.

At first it would seem that for this purpose we must know the nature of the curve B A; and that thus all the uncertainties which perplex us in regard to the gradual attenuation of the air would complicate our inquiry. But, fortunately, it turns out that these considerations are eliminated, and that the strict computation of the parallax is more simple and more direct than the ordinary approximate computations.

Having joined M with E, the centre of the earth, E M B is the true parallax. Continue M B and D A to meet perpendiculars let fall upon them from E. Then it is a well-known property of rays refracted by concentric strata that the ratio of these perpendiculars E K and E L is that of the indices of refraction of the strata at B and A inversely.



Now the index of refraction of the air at A can be obtained by direct experiment, or can be computed from the states of the barometer and thermometer, so that the ratio of E K to E L is known. Let this ratio be denoted by *i*.

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Then we have

$$\sin EMK = \frac{EK}{EM} = i\frac{EL}{EM} = i\frac{EA}{EM}\sin ZAD.$$

But $\frac{EA}{EM}$ is the sine of the moon's horizontal parallax, on the supposition of there being no atmosphere; and, therefore, it appears that the sine of the actual parallax is obtained by multiplying the sine of the horizontal parallax by the sine of the observed zenith distance and by the index of refraction of the air.

The true argument for parallax is thus the observed, and not the corrected, altitude, the formula being

$$\text{sin par.} = i \,.\, \text{sin Hor. Par.} \,.\, \text{sin app. Zen. Dist.}$$

On comparing this formula with the usual one, we find that the coefficient *sin corrected Zen. Dist.* has been replaced by *i . sin observed Zen. Dist.;* these two coefficients would be identical if the strata of the atmosphere were plane instead of spherical, so that the true computation must give a result differing more and more from the usual one as the angle A E B becomes larger, and we may expect the greatest deviation to be when the moon is in the horizon. When the apparent zenith distance is 90° the corrected is about 90° 33′, and the sine of the corrected zenith distance, instead of being greater, is actually less than that of the apparent. For the purpose of roughly estimating the inaccuracy of the common formula we may take these sines as equal to each other; now the index of refraction of air at its mean density is $\frac{3405}{3404}$ nearly; and, therefore, for planets apparently on the horizon, the parallax exceeds that quantity which is given in the almanacs as Hor. Par. by $\frac{1}{3404}$th part of itself; which gives in the case of the moon at its mean distance almost exactly one second of arc.

The strict computation of the parallax would be at once effected, if, instead of the horizontal parallax, as given in the almanac, which is half of the angle under which the earth would be seen from the moon were there no atmosphere, we had the true horizontal parallax, or that angle which the earth, as magnified through the atmosphere, actually does subtend at the moon. The sine of this augmented horizontal parallax would then have to be multiplied by the sine of the observed zenith distance. It is true that such a table would serve strictly only for observations made in the mean state of the air; but the variations in the index of refraction scarcely affect the result; a change of one inch in the barometer only producing a change of 0″·03 in the horizontal parallax. With the present arrangement we can easily obtain the strict result by adding 1285 to the seven place logarithm of the parallax.

From this investigation it appears that, in reducing observations made on the occultation of fixed stars by the moon, it is scarcely necessary to take the actual amount of refraction into account at all: at most, in observations on moon-culminating stars, the differential refraction is required.

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Waarnemingen gedaan te Manado ter bepaling van de Geographische lengte dier plaats, door S. H. De Lange en G. A. De Lange, Geografische Ingenieurs.

(*Communicated by the Astronomer Royal.*)

In the *Monthly Notices* for December last a brief account was given of a series of astronomical operations by the author of this paper, S. H. De Lange, in conjunction with his colleague, G. A. De Lange, having for their object the determination of the longitude of Batavia, chiefly by observations of the zenith distance of the moon. The most important feature of these operations consisted in the mode in which the zenith distance of the moon was determined. This was effected by first observing the moon, and then turning round the telescope in azimuth without unclamping it, so as to bring the star of comparison into the field of view. The operations for determining the longitude of Manado appear from the paper communicated on the present occasion to be precisely similar to those employed in the case of Batavia. The author, however, remarks, that the individual results are not so consistent in the present case,—a circumstance which he is inclined to attribute to the exposed situation of the telescope, and its consequent liability to be shaken by high winds. In reducing the observations of the moon's zenith distance, Adams's correction of the moon's parallax was employed. The observations extend from May 28, 1852, to January 21, 1853. As in the former paper, no definitive value of the longitude is given.

ERRATUM.

Page 49, line 2 from top, *for* Lieut. G. M. Gilliss, *read* Lieut. J. M. Gilliss.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *April* 13, 1855. No. 6.

M. J. JOHNSON, Esq. President, in the Chair.

Rev. Wm. Selwyn, Ely, and
W. R. Vines, Esq., Ealing, Herts,
were balloted for and duly elected Fellows of the Society.

Discovery of a New Planet. By M. Chacornac.

Admiral Smyth has received a letter from M. Chacornac announcing his discovery of a new telescopic planet of the eleventh magnitude at the Imperial Observatory, Paris, on the 6th of April. The following positions of it were obtained by a comparison with the star 25438 in the Catalogue of Lalande:—

1855.	Paris M.T. h m s	Right Ascension. h m s	Declination. ° ′ ″
April 6	13 10 49·6	13 39 50·02	−7 28 7·8
	28 5·6		
	15 32 53·9	13 39 45·19	−7 27 24·2
	51 40·1		

This discovery was also announced in a printed circular issued by M. Le Verrier, Director of the Imperial Observatory.

Elements of Chacornac's Planet. By M. Lesser.

Epoch, 1855 April 20·45093 M.T. Berlin.

	°	′	″	
M	38	48	53·0	
π	157	51	19·7	Mean equinox, 1855·0
Ω	184	1	45·3	
φ	6	34	5·0	
i	5	10	10·5	
log *a*	0·426961			
μ	2·909565			

These elements are calculated from the following observations:—

1855.	Berlin M.T. h m s	R.A. ° ′ ″	Decl. ° ′ ″	
April 6	14 44 14·0	204 57 5·3	−7 27 58·1	Paris
13	10 2 54·1	203 36 35·4	−6 38 57·2	Vienna & Berlin
20	10 49 20·7	202 12 53·3	−5 49 36·6	Berlin

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Discovery of a New Planet. By M. Luther.

Mr. Hind has received a letter from M. Luther, announcing his discovery of a new planet of the eleventh magnitude on the 19th of April, at the Observatory of Bilk. The following are two of its positions as provisionally reduced:—

1855.	Bilk M.T. h m	R.A. ° ′	Decl. ° ′
April 19	13 30	181 14	−5 11
20	10 27	181 6	−5 11
	Daily motion	−9	0

Discovery of a New Comet (*Comet II.*, 1855). By M. Schweizer.

On the 11th of April, about 11 o'clock in the evening, Dr. Schweizer, Astronomer, attached to the Geodesical Observatory of Constantine at Moscow, discovered a small telescopic comet, for which he obtained the following approximate positions by reference to Argelander's Star Charts:—

1855.	M.T.	Comet's R.A. ° ′	Comet's Decl. ° ′
April 11	11	184 40	−17 20
15	11	182 20	−13 40

Its daily motion is accordingly at present about,—

	′
In R.A.	−35 0
In Decl.	+55 0

On the Method of Observing the Positions of Spots on the Sun, and of Deducing their Heliographical Longitude and Latitude, adopted at Redhill Observatory. By R. C. Carrington, Esq.

"No change has been made in the mode of observation described in No. 5 of vol. xiv. of the *Monthly Notices*, but an improvement of the process of reduction has been introduced, which it is the object of this second communication to explain.

"Referring generally to the former description, and as before denoting by the letters A_1, A_2, B_1, B_2, the instants of contact of the sun's limb with the wires A *a*, B *b*, and by the letters *a*, *b*, those of a spot,

let $$A = \tfrac{1}{2}(A_1 + A_2), \quad B = \tfrac{1}{2}(B_1 + B_2)$$

also let $$\tan \alpha_1 = \frac{B_2 - B_1}{A_2 - A_1}, \text{ and } \tan \alpha_2 = \frac{b - B}{a - A} \cdot \operatorname{cotan} \alpha_1,$$

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then will $\alpha_1 + \alpha_2 + \iota$ be the angle of position of the spot on the disk from the north point reckoned towards the east, ι being the correction due to the inclination of the sun's path to a parallel of declination, and very nearly equal to four times the hourly increment of declination given in the *Nautical Almanac*, with its proper sign. If we denote the distance of the spot from the centre of the disk, expressed as a decimal of the radius, by $\frac{r}{R}$, we shall have

$$\frac{r}{R} = 2 \cdot \frac{b - B}{B_2 - B_1} \cdot \operatorname{cosec} \alpha_2, \text{ or } = 2 \cdot \frac{a - A}{A_2 - A_1} \cdot \sec \alpha_2$$

the first or the second expression of the two being used, accordingly as $b - B$ is greater or less than $a - A$.

"Let (R) be the sun's tabular semidiameter expressed in minutes of arc, and let $\varrho' = \frac{r}{R} \cdot (R)$, then will ϱ, the angular distance of the spot on the sun's body from its apparent centre, be found from the expression $\varrho = \sin^{-1} \cdot \frac{r}{R} - \varrho'$, the angle $\varrho + \varrho'$ being taken out to minutes and tenths.

"The quantities $\alpha_1 + \alpha_2 + \iota$ and $\frac{r}{R}$ give the means of forming a diagram of the appearance of the disk.

"*Second Step.*—Let the inclination of the sun's equator to the ecliptic be denoted by I, and the longitude of its ascending node by N. (The provisional values used at Redhill for 1854·0 are $I = 7° 10'$, and $N = 74° 30'$.) It is convenient to tabulate, for every degree of argument, the angles

$$\beta = \tan^{-1} (\cos \odot \cdot \tan \omega), \qquad \gamma = \tan^{-1} (\cos \overline{\odot - N} \cdot \tan I),$$

$$\alpha = \tan^{-1} (\tan \overline{\odot - N} \cdot \cos I), \quad \delta = \cos^{-1} (\sin \overline{\odot - N} \cdot \sin I),$$

where α and δ are respectively the heliographical longitude and north polar distance of the earth.

Let

$$\chi = \overline{\alpha_1 + \alpha_2 + \iota} + \overline{\beta + \gamma}$$
$$\cos \varphi = \cos \varrho \cdot \cos \delta + \cos \chi \cdot \sin \varrho \cdot \sin \delta,$$
$$\sin \theta = \sin \chi \cdot \sin \varrho \cdot \operatorname{cosec} \varphi$$

then will $\overline{\alpha - \theta}$ and φ be the heliographical longitude and north polar distance of the spot.

"It will be remarked that no account is here taken of the effect of refraction. The reason is, that a little experience convinced me that, unless its effect could be allowed for in current reductions in some very simple manner, it was better to omit it; and I have not been able to devise any mode of computing a correction for it at all corresponding in brevity to the above formulæ.

"The brevity of the formulæ now given will best be shown by an example, and for this purpose we will take an observation of the principal nucleus of the large spot seen in January last.

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"The mean of three passages on Jan. 18th gave the following numbers:—

A_1	19h 36m 28s·867	Slow 55s·5	Longitude 41s·3 W.
B_1	36 43·867	G.M.T. of obs.	1855, Jan. 17d 23h 51m.
a	37 40·133	$\odot$ = 297° 53′·7	β = +11° 28′·8
b	38 5·067	N = 74 30·7	γ = − 5 13·2
A_2	39 48·067	$\odot$ − N = 223 23·0	ι = + 2·1
B_2	19 39 59·800	α = 43 9·5	δ = 94 55·0

$B_2 - B_1$ = 195·933	Log = 2·29211	Log tan α_1 = 9·99282
$A_2 - A_1$ = 199·200	Log = 2·29929	α_1 = 44° 31′·6

$b - B$	= − 16·767	Log $(b - B)$	= −1·22445
$a - A$	= − 28·334	Log $(a - A)$	= −1·45231
Log $(a - A)$	= 1·45231	Diff.	= +9·77214
Log $(A_2 - A_1)$	= 2·29929	Log tan α_1	= 9·99282
Diff.	= 9·15302	Log tan α_2	= +9·77932
Log sec α_2	= 0·06708	α_2	= 211° 1′·9
Log 2	= 0·30103	α_1	= 44 31·6
Log $\frac{r}{R}$	= 9·52113	ι	= + 2·1
Log (R)	= 1·212	$\beta + \gamma$	= + 6 15·6
Log ϱ'	= 0·733	χ	= 261 51·2
Log cos ϱ	= 9·97488	$\varrho + \varrho'$	= 19 23·4
Log cos δ	= −8·93301	ϱ'	= 5·4
Log n_1	= −8·90789	ϱ	= 19 18·0
Log cos χ	= −9·15140	Log sin χ	= −9·99559
Log sin ϱ	= 9·51919	Log sin ϱ	= 9·51919
Log sin δ	= 9·99840	Log cosec ϕ	= 0·00357
Log n_2	= −8·66899	Log sin θ	= −9·51835
n_1	= −0·08089	θ	= −19° 15′·6
n_2	= −0·04667	α	= 43 9·5
$n_1 + n_2$	= −0·12756	$\alpha - \theta$	= 62 25
Log $(n_1 + n_2)$	= −9·10571	ϕ	= 97 20

"The result of the calculation is accordingly,—

Angle of position, 255° 35′·6	Dist. from centre, 0·3320
Heliographical long. 62° 25′	Latitude, 7° 20′ South

"As was mentioned in the Annual Report lately published, this process of reduction has been applied at Redhill to all the observations of spots which were obtained during the past year, and a tolerably continuous catalogue of positions and of corresponding diagrams is consequently accumulating. As I feel well satisfied after a full trial with the method now put forth, and think it improbable that a shorter one can be devised, I

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venture again to express a hope that those who interest themselves in this subject will give my method a trial.

"The following series of observations of a small spot may be useful as exhibiting the degree of accuracy of the results obtained:—

1854.	G.M.T. h m	Pos. ° ′	Dist.	H. Long. ° ′	H. Lat. ° ′
Aug. 25	0 12	123 2	·9399	189 24	S. 10 14
26	0 51	126 44	·8445	203 23	10 20
27	1 32	132 9	·7022	218 10	10 3
28	0 41	141 24	·5481	231 56	10 13
29	0 38	159 58	·3912	246 19	10 23
30	0 32	195 9	·3031	260 29	10 23
31	1 6	235 23	·3611	275 6	10 19
Sept. 1	0 24	256 34	·5033	288 54	10 8
4	The spot had entirely disappeared.				

"Such series, it is needless to state, are suitable for a basis for the further correction of the assumed elements of the position of the sun's poles. I have, of course, several such series already observed, but they are not yet sufficiently numerous to warrant the adoption of any certain corrections of the assumed values of I and N. The problem of correcting the assumed position of the sun's axis from such data is similar to the case of finding the error of position of the polar axis of an equatoreal from three or more observations in north polar distance of the same star at different hour-angles.

"My observations during 1854 suggest one or two remarks which may be appended here, although they are, I hope, by no means all that they will ultimately afford.

"1. The year 1854 has been one in which the number of outbreaks has been very few,—indeed, approaching a minimum. Now the reduced list of positions includes no spot more distant from the equator than 19° 30′ to the north, and 17° 0′ to the south. This circumstance appears to afford grounds for concluding that when the eruptive forces are comparatively quiet, the limits of parallel are also much contracted.

"2. On the 29th of September I observed a moderate-sized spot in position 110° 17′ from the north point, distance from centre = 0·9323, the penumbra of which included two detached nuclei; on the following day, the 30th, it was observed that the 'following' nucleus was outside the penumbra. Careful diagrams were made at the time on both days. I cannot reconcile this observation with the commonly received idea of the relation of nucleus and penumbra. As an isolated instance, it does not appear to call for more than being recorded at present; or rather I may say that, until other instances shall be met with, it appears to me to be premature to state the inferences which the observation seems to suggest.

"3. Throughout the year 1854 there has been a continual

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tendency to recurrence of spots on the same parallels of latitude, both of individual spots on almost precisely the same parallel, and of groups within the same general limits. The parallels 71° to 75° Heliog. N.P.D. have been but rarely visited; the parallels 77° to 85°, frequently; from 86° 30′ to 89° 30′, not at all; from 90° to 92°, very rarely; from 92° to 94°, not at all; from 95° to 103°, frequently; from 103° to 107°, very rarely. In connexion with this circumstance, I would suggest whether it be not worth while to examine into the possibility of an explanation arising from the hypothetical revolution of an inner body, on which may be situated the volcanic centres of eruption, in a shorter period than the revolution of the envelope on which we see the effects in the form of ruptures of continuity. It will appear to any one who considers this suggestion that, to bring it to the test of facts, there will be required a collection of observed positions of nuclei on the same parallel, together with the times of their first appearance on the envelope; and accordingly, I would recommend fellow-labourers on this subject to be careful to record the occurrence of outbreaks which they may succeed in ascertaining to have taken place between two consecutive days."

Remarks on the Orbits of α Centauri and σ Coronæ Borealis.
By Capt. W. S. Jacob, Astronomer at Madras.

In the *Monthly Notices* for January are given the elements of two orbits for *α Centauri*, contributed by Messrs. E. B. Powell and J. R. Hind. The latter, probably, represents the observations pretty fairly, but the former can scarcely be said to do so, as it makes the observed distances before 1846 all too great, and after that epoch all too small, while even in the angles there is in a general way the same opposition of signs, though not quite without exception; in such a case, it is evident that the sum of the errors or of their squares must be capable of considerable reduction.

"But, in fact, the agreement of the orbit with observation is no proof that it is even an approximation to the true one: where we have given only a small portion of the periphery of an ellipse, it is evident that with a very slight change of curvature we may adapt it to many different ellipses; and, even when we are pretty sure of the dimensions of the apparent ellipse, it may happen that a very slight change of position of the projected focus may cause a great variation in the true elements. This is the case in the present instance: I have computed a number of orbits for this star, but for some years have given up the attempt, as the data are insufficient; though we can lay down the apparent orbit very closely, we cannot as yet get even a decent approximation to the true one.

"The reason is this: La Caille's observations in 1751, though not, of course, minutely accurate, enable us to fix the periodic time within a small quantity; since the stars had returned to the same

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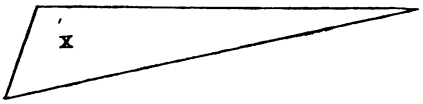
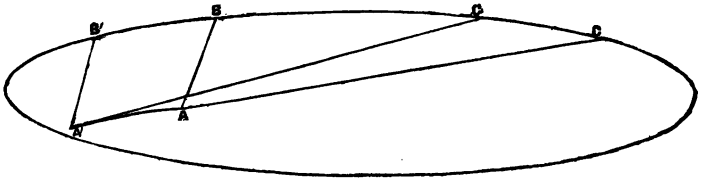
relative position about 1830, the period must evidently be about 79 years, within a year or two either way; and as we know pretty exactly the area now described annually by the radius vector, we can also fix nearly the total area of the apparent ellipse. The path described from 1834 to 1854, the only time within which trustworthy observations are available, is shown on the slip marked X, and is of such extent and small curvature that it must



include the extremity of the minor axis, and having given the curvature at this point and the area, the dimensions of the ellipse are fixed. It appears that the axes must be about 28″·80 and 6″·86, as shown in the lower diagram. If we now try to fit the slip X to the periphery of the ellipse, we find it may take the position A B C; but, by reason of the slight change of curvature



in so elongated an ellipse for a considerable distance on either side of the minor axis, it may also be *slided* round the curve as far as A′B′C′ without material error, the locus of the principal star, or projected focus, being the thick line A A′, and, with the exception of not being *very* likely to fall quite close to either extremity, it may as well occupy one part of the line as another, and the observations will be equally well represented whatever point be fixed on. This, then, allows a range in per. pass. of from 1858·5 to 1867·8, and in *e* from ·54 to ·96; while *a* may range from 15″ to 31″. These limits may be a little further extended by slight variations in the dimensions of the apparent ellipse, but they will be somewhat narrowed by the end of the current year; for if the first position be nearly true, the places at 1856·0 will be 309°·0 and 3″·75, and if the second, 312°·6 and 3″·45,—a difference which observation would be sure to detect; still we shall not be able to get a good approximation until the extremity of the ellipse has been reached, or nearly so.

"σ *Coronæ Borealis.*

"In the same Number (page 90) is given, also by Mr. Powell, an orbit for σ *Coronæ Borealis*, which appears from the comparisons cited to agree closely with observation. There are, however, a few epochs omitted where the agreement would not be

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quite so close; it may also be doubted whether the plan of grouping together several epochs into one is quite legitimate, for in the case of such quick-moving stars the mean of angles extending over two years would not correspond to the mean of the times.

"The following elements will be found to represent the observations pretty fairly, but they will admit of considerable variation:—

τ = 1831·17
ϖ = 107° 13′
Ω = 1 57
λ = 101 57
γ = 46 47 cos = [9·83552]
e = ·3088
P = 195ʸʳ·12 a = 1″·845
α = 2″·717

Comparison.

Date.	Observer.	θ_o	$\theta_c - \theta_o$ ∠	$\theta_c - \theta_o$ arc.	ϱ_o	$\varrho_c - \varrho_o$
		° ′	′	″	″	″
1781·79	H	347 32	− 46	·038		
1802·74	,,	11 24	+ 85	·058		
1819·60	Σ	50 18	+183	·081		
1821·30	H & S	65 15	−302	·129		
1822·67	Σ	61 0	+300	·123		
1823·47	H & S	72 56	−199	·082	1·45	−·055
1825·44	S	77 31	+104	·040	1·48	−·160
1826·77	Σ	89 0	−171	·065	1·30	+·012
1828·50	H	92 6	+183	·069		
1830·28	H	105 5	− 36	·014	1·22	+·092
1830·52	D	107 17	− 93	·035		
1830·76	Sm	107 36	− 37	·014	1·30	+·014
1831·34	D	111 32	− 93	·035	1·57	−·250
1831·36	H	108 46	+ 79	·030	1·39	−·070
1832·37	Sm	114 54	+ 21	·008	1·40	−·054
1832·55	D	115 57	+ 13	·005		
1833·36	,,	120 37	− 43	·017	1·30	+·084
1833·58	Sm	120 42	+ 14	·006	1·20	+·190
1835·50	,,	130 54	− 86	·033	1·40	+·048
1839·67	,,	145 6	− 6	·003	1·60	+·064
1843·35	,,	155 54	− 2	·001	1·80	+·015
1846·21	*j*	161 58	+ 49	·027	2·25	−·300*
1853·14	,,	177 54	− 54	·034	2·18	+·040
1853·35	Powell	175 12	+130	·083		
1854·05	*j*	177 52	+ 45	·029	2·22	+·036

* Only a single night's measure, and not very good.

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"N.B. As it is desirable that the same observer should be always designated by the same letter, and as J has been already appropriated by Manuel Johnson, it will be better to keep to *j* for designating my observations."

5 *March*, 1855.

Description of an Observatory erected at Grantham.
By J. W. Jeans, Esq.

"I erected this observatory about five years since. The transit-room is placed between the two roofs; the timbers which carry the floor-joists being firmly spiked to the rafters of the roofs, abutting against their tye-beams; the pedestal of the transit is quite free from the floor; the walls are of wooden frame-work covered with sheet-iron, canvassed and papered inside; the roof ditto ditto, but having felt under the iron, which is galvanised. The equatoreal-room is constructed in a similar manner; the joists of the floor being carried by the outer or western purling of the roof, at one end, passing quite clear of the inner or eastern purling, and being carried at the other end by timber uprights passing down to the party wall. A large box full of sand is attached to the eastern purling, upon which the equatoreal rests, quite independent of the floor, and but little affected, except by the vibration of the house itself; the walls of this part are lined with boards; the roof consists of light wood framing, covered with felt and sheet iron; it has double shutters, about 22 inches wide, opening quite across from curb to curb, in four sections closed at top by a square shutter, which can be raised so as to allow observations quite in the zenith. The curb is made of American oak, top and bottom double, lined with one-eight inch sheet-iron, and running on cast-iron balls. I cannot state the cost of erection, as it was partly constructed of old materials, and I did a great deal of the work (nearly the whole of the dome) myself.

"The transit is by Cary, 2 ft. long, 2 in. aperture, 6 in. circle, divided to 30′, reads to 1″; striding level; it is very steady to its adjustments, and performs very well. The equatoreal also by Cary, $3\frac{7}{8}$ in. aperture, 5 ft. 9 in. solar focus, circles 9 in. diameter, H. C. divided to 1^m reads to 2^s, D. C. divided to 15′ reads to 30″; it performs very fairly. The clock-work to it is of mine own adaptation. The clock is an old one, Grantham make, dead beat, and furnished with a wooden compensating pendulum-rod, of mine own construction; its rate is very steady and good. My position, according to mine own observation, is

Lat. 52° 24′ 52″ North
Long. 0 39 0 West.

[The foregoing description was accompanied by a complete and very neatly executed drawing of Mr. Jeans's observatory.—Editor.]

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On the Theory of M. Foucault's Gyroscope Experiments.

By the Rev. Baden Powell, M.A. F.R.S., &c.

The remarkable experiments of M. Foucault for exhibiting both a new direct proof of the earth's rotation, and the indirect effect of it in the "orientation" of the axis of a disk influenced only by mechanical rotation, have excited great interest; and thus an attempt to elucidate and simplify the *theory* of them may not be regarded as useless, especially as that *theory* has been found in some degree obscure and complex, more particularly as treated on the principle of M. Poinsot's theory of "couples." See *Comptes Rendus*, tom. xxxv.

The apparatus (or "gyroscope") consists essentially of a circular metallic disk, heavily loaded round its circumference, rotating on an axis, whose ends turn with the most perfect freedom in the opposite points of a brass circle, itself supported at the intervening opposite points on pivots in a second brass circle, suspended in a vertical plane by a thread without torsion at the top, and resting on a pivot at the bottom, both attached to an outer vertical frame. The motion of either of these circles can be suppressed, so that the axis of the disk can move either in altitude or in azimuth only, or freely in both (see fig. 1).

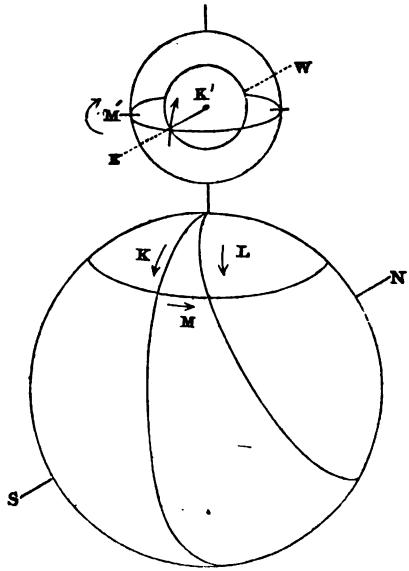

Fig. 1.

In these experiments the most material mechanical condition is what M. Foucault terms "the constancy of the plane of rotation,"—that is, the loaded disk having an immense rotatory

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velocity communicated to it by the action of a train of wheels before it is placed on its pivots, retains, by virtue of its great acquired momentum, this motion *in the plane in which it was communicated*, independently of gravitation or any other circumstances,—so much so, as to resist with considerable force any attempt to place it in a different position.

In the following brief explanation it may be necessary to premise,—

1. The principle of the composition of rotatory motions, or that if a body be rotating about a given axis, and a tendency to rotate about another axis at the same time be given to it, the two rotations compounded will give a resultant rotation about a new axis inclined in a direction between the two former, whose position is determined by the well-known theorem, for which see Mr. Airy's tract on Precession.

2. The inverse principle of the resolution of rotatory motion, as applied by Euler to the rotation of the earth at any point on its surface, where the whole rotation being in a plane passing through the parallel of latitude of the place (which, for brevity, I call "the plane of latitude"), and the rotation for a given time (L) in that plane, it is resolved into two components, one in a circle passing through the place perpendicular to the meridian (K), the other in a circle (M) cutting the last at right angles, and whose centre is the place of observation. It is easily seen that the component (K) is *initially* proportional to the *cosine*, and (M) to the *sine* of the latitude (see fig. 1).

3. In any composition of forces p and q, of which r is the resultant, if the same action be repeated with p and r as components, the new resultant r_1 will lie between p and r. If it be again repeated r_2 will lie between p and r_1, and so on, till ultimately r_∞ will coincide with p.

Case I.—The disk rotates, being in perfect equilibrium, and free to move both in altitude and azimuth (see fig. 1).

Let us suppose the disk to have its axis in the first instance horizontal, and pointing east and west.

Then the rotation of the earth carries it along with the apparatus, and, if at rest, would cause it to incline towards the east in the plane of the latitude.

But, owing to "the constancy of the plane of rotation" the disk tends to retain parallelism to its original plane; and being perfectly free to move with respect to the apparatus, and wholly free from the influence of gravitation, it has a relative apparent motion equal and opposite to that of the earth in the plane of the latitude (L), the components of which in altitude (K′) and in azimuth (M′) are exhibited by the moveable circles of the apparatus respectively.

The motion in altitude would be inconvenient to observe, as it changes its plane continually; that in azimuth is easily observed, by means of a horizontal microscope with focal wires, directed to a small graduated scale on the edge of the vertical circle.

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These motions continue with the rotation of the earth as long as the rotation of the disk is kept up: and the *rate* of the azimuthal motion is easily seen to be proportional to the *sine of the latitude* of the place, as in the case of the free pendulum.

Case II.—The disk rotates, having its axis horizontal, and free to move only in azimuth.

Let the plane of the disk (d) be at first parallel to the meridian (see fig. 2), or have its axis pointed E. and W.

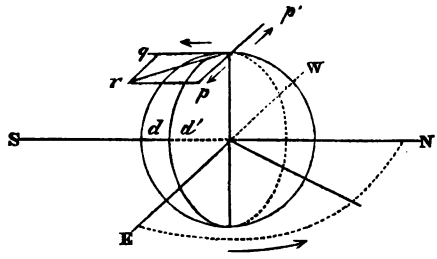

Fig. 2.

Then the constancy of the plane of rotation opposing the effect of the earth's rotation in the plane of the latitude tends to make the disk incline in that plane from east to west, initially in the direction (p') of the tangent to the arc which its upper extremity would describe. But the vertical diameter of the disk being fixed by the apparatus relatively to the earth, offers an equal and opposite reaction (p).

This has to be compounded with the rotatory motion of the disk (q), (suppose towards the south, at the upper part), which will give a resultant rotation (r) in a plane between the two; or the disk will take a new position (d'), to which (r) is a tangent, in a plane inclined to that of the meridian, its axis turning in azimuth from east towards north.

In this new position the same action will be repeated, the components being now (p) and (r), and the new resultant (r_1) will place the axis between the former position and the north, and so on, till it ultimately points to the north, coinciding with the meridian, in which position it will continue.

If the rotation had been the opposite way it is easily seen; a similar construction would give the disk turning from north towards east. And in either case, when in the final position, the rotation will be in the same direction as that of the earth.

Case III.—The disk rotates, having its axis free to move *only* in altitude, and in the plane of the meridian.

In the first instance, suppose the axis horizontal, and the rotation towards the east at the upper part (see fig. 3).

The disk, if free, would have an apparent motion opposite to that of the earth, or the eastern extremity of its horizontal diameter

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would have a motion upwards in the plane of the latitude, whose initial direction (p^1) is the tangent of the arc it would describe.

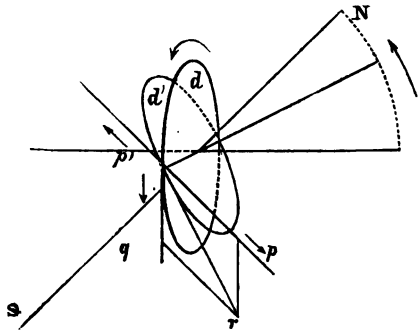

Fig. 3.

But this motion is counteracted by the apparatus giving an equal and opposite reaction downwards (p). This, combined with the rotation (q), gives a resultant (r) between them, in consequence of which the disk (d) takes a new inclination (d'), its axis ascending at the north end, till ultimately it becomes parallel to the axis of the earth, when the rotation of the disk will be in the same direction as that of the earth. If it had been originally rotating in the opposite direction, it is easily seen in the same way that it would come round to the same position.

Report of the Director of the Imperial Observatory of Paris on a Plan for Improving the Organisation of that Establishment.

In accordance with a decree of the Government, the Director of the Imperial Observatory of Paris has recently drawn up a Report on a proposed plan of operations to be pursued at that establishment with the view of placing it on a level with the other Observatories of the first class in Europe. The details of the plan are embodied in a Report recently addressed to the Emperor by M. Fortoul, Secretary of State for the Department of Public Instruction. The contents of this Report, of which the following is the substance, are classed under seven distinct heads.

1. *Meridian Observations.* — Operations of this kind were long imperfectly attended to in France. The first transit instrument was erected at the Paris Observatory only in the year 1800. Since that time foreign establishments have accomplished from year to year considerable progress. In order that the Observatory of Paris may maintain itself on a level with them, it is indispensable that an improvement be effected in the instruments available for that purpose, as well in regard to optical power as in

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regard to stability. It will be necessary, besides, to construct a large meridian circle, and to economise the resources presented by dynamical electricity, in order to assure to the observations all the precision of which they are susceptible.

2. *Extrameridional Observations.*—The Observatory of Paris not having yet taken a sufficient part in labours of this kind, it will be necessary to establish an entirely new branch of service, in order to respond to the important interests which are dependent on them.

3. *Comparison of Observations with Theory.*— Observations were formerly published in their rude state, without appending to them the labour of discussion which acquires for them a scientific value. It is not so in the present day: foreign establishments in Europe and America have followed the example given by England twenty-five years ago, and publish only discussed observations.

The Observatory of Paris is one of those which have not yet adopted this improvement. It will be the more difficult to realise it in practice, inasmuch as the ephemerides which have served as the basis of reduction are still in an imperfect state. A plan in reference to this object, based upon eminently scientific principles, is stated to have been proposed by the Director of the Imperial Observatory.

4. *Relations with the Public Service.*—It is extremely desirable that the Observatory of Paris, after having made due provision for the advancement of science, should so dispose its resources as to enable the Government to profit by them. The study of the chronometers of the Imperial Marine might thus be improved, and the same advantage might be accorded to the Mercantile Marine. The different astronomical and meteorological instruments used at sea might also be compared at the Observatory.

The exact time might be sent daily by telegraph to the different parts of the empire, where it is to be obtained at present only imperfectly and for money. On extraordinary occasions it might be desirable to transmit to them atmospheric instructions, which might be necessary for the security of the shipping.

In order to determine with greater accuracy the principal data upon which the map of France has been constructed, it is suggested to unite the resources of the Observatory with those of the Ministry of War. It would be also desirable to transmit the exact time to the principal points of the Metropolis, &c. &c.

5. *Operations connected with Physical Science.*—The progress of astronomy is intimately allied to that of physics, and the latter tends to approximate towards astronomy as well by the services which it renders as those which it receives by borrowing its instruments and methods.

The establishment at the Observatory of a department of physical observations placed under the direction of a *savant* specially appointed for that purpose, is indispensable in the present day, not less for the wants of astronomy than for the solution of the great questions of modern physics. Such an establishment would

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be extremely desirable for maintaining the optical instruments in a high state of efficiency, and for assisting artists in the fabrication of large glasses.

The use of telegraphic apparatuses, the employment of electricity in daily observations, as well as the relations of the observatory with the public service, necessarily demand the attention of a person well acquainted with experimental science.

It would also devolve on the physicist to investigate the advantages which might accrue to the sciences of astronomy and meteorology from the application of photography. Thus the photographic and instantaneous observation of the solar disk, by rendering available a source of light, the extreme intensity of which has hitherto been an object of dread, would assuredly furnish precious materials respecting the physical constitution of the sun.

The vertical has hitherto been considered as fixed, and yet theory indicates that gravity undergoes small variations both in intensity and direction under the influence of the combined attractions of the sun and moon. The investigation of apparatuses capable of indicating this kind of influence, and thereby furnishing new determinations of the masses of the sun and moon, would form an admirable subject of study to the physicist.

Finally, the velocity of light and of electricity has been established in the present day upon the surface of the earth. The same is true in respect to the rotation of the earth, a phenomenon of which the demonstration lately belonged exclusively to astronomy, but is now accessible to physics, which has furnished a proof of its existence that is sensible to all eyes. Still it may be affirmed that the labours undertaken with respect to this object, notwithstanding the just interest which they have excited, are yet only commenced. They might be resumed at the Observatory, receive there the last degree of precision, and thus be made to furnish important data for a knowledge of the solar system. This section concludes with recommending M. Leon Foucault to the office of Physicist to the Observatory.

6. *Meteorological Operations.*— It is also to the Physicist of the Observatory that the direction of the meteorological operations would belong, and this is not the least important part of the labours intrusted to him.

The establishment of model instruments, and of self-operating instruments; researches on the temperature of the air; on hygrometry, on pluviometry, on winds, on atmospheric electricity, on terrestrial magnetism, on atmospheric phenomena, such as halos, auroræ boreales, &c.: all this branch of meteorological observations is still to be created at the Observatory of Paris.

Even although confined to a single station, these studies would be of great utility. They would offer still greater interest if it were possible to extend them over the whole surface of the earth, and to trace across continents and seas the propagation of great atmospheric waves. It would be necessary for this purpose to

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collect together the materials established at a great number of stations, and to institute a comparison between them. The electric telegraph promises in the present day to accomplish this important work with the most complete success.

7. *Scientific Administration.*—This section includes theoretical instruction, and also practical instruction in the art of observation, the care of publications and the charge of a vast and growing correspondence. In order to assure the improvement of these different departments it would be desirable to increase the salaries of the officials of the Observatory who are not remunerated in proportion to the difficulty and importance of their labours.

By an Imperial decree subsequently issued, the situation of Physicist of the Observatory has been instituted, and M. Leon Foucault has been appointed to this new office. The salaries of the astronomers connected with the Observatory have also been increased in pursuance of the same decree.

ERRATUM.

Page 161, line 3 from bottom, *for* agreement, *read* argument.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *May* 11, 1855. No. 7.

M. J. JOHNSON, Esq. President, in the Chair.

Rev. F. Silver, Norton, near Market Drayton, Shropshire, was balloted for and duly elected a Fellow of the Society.

On the Measured Distance of 70 *Ophiuchi.*
By the Rev. W. R. Dawes.

"The fine double-star 70 *Ophiuchi*, being quite within the optical power of comparatively small telescopes, has not received from me that constant attention which, perhaps, from its great intrinsic interest it deserves. As, however, the elements of the orbit recently computed by Mr. E. B. Powell give a continued *increase* of distance, while the measures of several observers show that that element is *decreasing*, I am induced to anticipate in this instance the publication of the results I have obtained, forming a portion of a large mass of observations, which, I hope, will before long be in the hands of the Astronomical Society.

"My mean results are the following:—

Pos.	Obs.	Nights.	Dist.	Obs.	Nights.	Epoch.
°			′			
118·83	15	3	6·804	16	3	1848·12
114·66	29	7				1853·60
			6·489	24	5	·68
113·71	18	4	6·339	14	3	1854·73

"These measures fully support the conclusion indicated by those of Captain Jacob, Mr. Miller, and Mr. Fletcher; and, indeed, do not differ widely from any of them. The decrease of distance to the extent of about 0″·4 or 0″·5 since 1848 seems to be well established.

"*Wateringbury, May* 1, 1855.

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Observation of an Occultation of Venus by the Moon.
By J. Ferguson, Esq.

(*Letter to Lieut. Maury, U.S.N., Superintendent of the National Observatory, Washington, communicated by Lord Wrottesley.*)

"I submit the following results and description of the occultation of the planet *Venus* by the moon, observed with the large equatoreal on the 18th of the present month. The following are the meteorological indications for the time of observation taken from the journal of the Observatory,—

Bar. 29·912. Therm. attached, 75°. Ext. therm. 70°·5.

"The night was sultry, with a damp atmosphere, but quite serene and clear, till within 15 degrees of the horizon, below which was a brownish haze, as of Indian summer. The dark limb of the moon was distinctly seen with the naked eye until after the occultation. After 8 o'clock the planet became ill-defined, having a reddish-purple tinge. The power of the eyepiece used was 120, with which the following times were observed:—

"At $8^h\ 37^m\ 30^s$ M.T. Wash.—The limbs were in contact. This time is uncertain two seconds, owing to the flame-like edge of the planet, the moon's limb being well defined.

"At $8^h\ 39^m\ 10^s$.—The planet was seen half its diameter within the limb of the moon, exhibiting no diminution of light or of magnitude, but showing as if it were on this, and not on the other side of the moon.

"At $8^h\ 39^m\ 38^s$.—The first diminution of magnitude was apparent, the inside or cut edge being straight and well defined, the planet still showing as if projected on the surface of the moon. The last two phases were observed with great care.

"At $8^h\ 40^m\ 7^s$.—The immersion, which was sudden, but not instantaneous.

"*U.S.N. Observatory, April* 19, 1855."

Account of the Operations for determining the Longitude of Fredericton, New Brunswick, by Galvanic Signals. Extracted from a Report to the Lieutenant-Governor.

(*Communicated by the Astronomer Royal.*)

"The Government of the United States has spared neither pains nor money in determining by the most approved astronomical methods, and by interchanges of upwards of one thousand chronometers, the difference of longitude between Greenwich and Harvard College Observatory, in order that the latter might

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serve as a point of reference in conducting the operations of the Coast Survey. By our telegraphic communication with Boston, and through the kind co-operation of Professor Bond and his assistants, we have, at a comparatively insignificant amount of trouble and expenditure, been enabled to avail ourselves of the labours undertaken for the above-mentioned purpose, and thus to ascertain the longitude of Fredericton with probably an equal degree of precision.

"It was originally intended to have an unbroken telegraphic communication between the Fredericton Observatory and that of Harvard University, but in consequence of the wires from the latter to the office in Boston being out of repair, Professor Bond found it necessary to trust to two excellent sidereal micrometers for the interval; and remarks, that on examination he was induced to believe that no greater error had arisen from this source than would have taken place had the communication been made from the room adjoining the transit instrument. Professor Bond's chronometers were carefully and repeatedly compared with his transit-clock and with each other, both before and after interchanging signals, so as to ascertain their error and rate; and at both observatories, on each day of operations, the meridian passages of a number of stars were observed, in order to obtain the error and rate of the transit-clocks.

"On the evening of the 23d of January, 1855, we received the first series of signals from Boston. Mr. Coolidge (Mr. Bond's assistant) commenced at an even minute by his chronometer, and sent us second-beats for fifty consecutive seconds. This was continued for ten successive minutes, beginning always at the even minute, and we carefully noted the times by our transit-clock. On examining all, we found that the times of the first signal would be as exhibited in

Table I.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	6 29 37·7	1 40·18	6 27 57·52
Cambridge	6 10 21·5	0 21·36	6 10 0·14
		Hence the difference of longitude	0 17 57·38

"On the evening of the 2d of February we took the initiative, and sent a series of signals to Boston, the result from which is given in

Table II.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	6 38 0	2 25·88	6 35 34·12
Cambridge	6 18 13·3	0 36·48	6 17 36·82
		Hence the difference of longitude	0 17 57·30

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"On the same evening we sent another series, and the result deduced from them is shown in

Table III.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	6 49 0·0	2 25·88	6 46 34·12
Cambridge	6 29 13·3	0 36·48	6 28 36·82
		Hence the difference of longitude	0 17 57·30

"On the same evening we received from Cambridge a series of signals, which give a result exhibited in

Table IV.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 4 23·6	2 25·88	7 1 57·72
Cambridge	6 44 37·0	0 36·48	6 44 0·52
		Hence the difference of longitude	0 17 57·20

"On the evening of the 10th of February we were again in telegraphic communication with Boston, and the result of the first series of signals which were sent from Fredericton and recorded at Boston is exhibited in

Table V.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 1 0	3 23·7	6 57 36·3
Cambridge	6 40 27·05	0 47·89	6 39 39·16
		Hence the difference of longitude	0 17 57·14

"The second series of signals on the same evening was transmitted from Boston and recorded at Fredericton, and the result is as shown in

Table VI.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 17 21	3 23·7	7 13 57·3
Cambridge	6 56 48	0 47·89	6 56 0·11
		Hence the difference of longitude	0 17 57·19

"We next sent a series of signals to Boston, the result derivable from which is given in

Table VII.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 43 0	3 23·7	7 39 36·3
Cambridge	7 22 27	0 47·89	7 21 39·11
		Hence the difference of longitude	0 17 57·19

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"We then received from Boston and recorded at Fredericton another series of signals (the fourth of the same evening), the result of which is shown in

Table VIII.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 51 21	3 23·7	7 47 57·3
Cambridge	7 30 48 (17·49 × 30·4 =)	0 47·89	7 30 0·11
	Hence the difference of longitude		0 17 57·19

"And, lastly, we received from Cambridge a single tap for the purpose of comparing clocks, and the result deducible from it is exhibited in

Table IX.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	8 7 21	3 23·7	8 3 57·3
Cambridge	7 46 48	0 47·89	7 46 0·11
	Hence the difference of longitude		0 17 57·19

"On examining the operations of February 10, it will be perceived that the second-beats of the Boston chronometer and the Fredericton transit-clock continued synchronous throughout, and, therefore, must have had the same rate. Hence the same clock-errors are applicable to the whole of the series for the evening. We may remark, that the results obtained from this last night's work are considered the most complete and satisfactory, and from them alone the difference of longitude would be $0^h\ 17^m\ 57^s{\cdot}18$. If, however, we take the mean of all the operations, the difference would be $0^h\ 17^m\ 57^s{\cdot}23$; and as Cambridge Observatory is $4^h\ 44^m\ 30^s{\cdot}66$ west of Greenwich, it follows that the longitude of Fredericton is $4^h\ 26^m\ 33^s{\cdot}43$ west of Greenwich. Converting the above time into arc, we have,—

Longitude of Fredericton	66° 38′ 21″·5
The Crown Land Department makes it	66 37 54
Difference	0 0 27·5

"This difference is smaller than could have been anticipated, or than we should have been warranted in assuming.

"J. B. Toldervy.
"W. B. Jack.

"*Fredericton, March* 5, 1855."

The following is an extract of a letter from Professor Jack to the Astronomer Royal, dated Fredericton, April 19, 1855, in reference to the foregoing determination:—

"Morse's machine was employed, and as the armature of the magnet at one end of the line has, on the connexion being made

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by means of the finger-key at the other, to move through a space not probably exceeding the $\frac{1}{200}$ part of an inch, and as the contact of the armature and magnet gives a distinct sound, which can be easily compared with the second-beats of the transit-clock, we cannot but think this method susceptible of greater accuracy than that which depends on watching the movements of a needle. Still greater accuracy could have been obtained had we been supplied with Bond's self-registering apparatus."

The following is an extract of a letter, dated April 23, 1855, received from George Hamilton, Esq. F.R.A.S., Egremont, near Liverpool, in reference to Mr. Elliott's invention of a Mechanical Imitation of Precession, alluded to in the last number of the *Monthly Notices*:—

"In the year 1842, Mr. Edward Davis, optician, then of Liverpool, now, I believe, of Shrewsbury, constructed for me an apparatus precisely the same in principle as Atkinson's, and precisely the same in form as Elliot's. I have used the apparatus since that time to illustrate precession of the equinox and nutation of the earth's axis. My apparatus was made after a model shown to me by Mr. Elliot, and, judging from the appearance of his apparatus, I should conclude that it had been constructed many years before."

Elements of Leucothea. By M. Bruhns.

Epoch, 1855. May 0·0, M.T. Berlin.

M	= 351° 16′ 22″·7	
π	= 212 55 38·7	Mean equinox, 1855·0
Ω	= 355 42 15·2	
i	= 7 37 35·9	
φ	= 14 17 36·9	
μ	= 694″·479	
log a	= 0·472232	

These elements were calculated from an observation made at Bilk, dated April 20; and two Berlin observations of April 27 and May 3.

Note relative to a Phenomenon seen in the Planet Venus. By the Rev. W. R. Dawes.

"In the *Monthly Notices* for March 1854, there is a notice of some observations of the planet *Venus*, when near her inferior conjunction, by Mr. Guthrie. The phenomenon he observed is

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very interesting; and it seems desirable that some more definite statement should be furnished by Mr. Guthrie respecting the observations, giving the exact *date*, the approximate *altitude* of the planet, or the *time* of observation from which the altitude and the degree of twilight might be computed.

"It is to be regretted that a notice of the observations was not published at the time, as it might have led to a repetition of them by other observers on subsequent opportunities.

"I confess that I have frequently examined *Venus* near her inferior conjunction, with the view of ascertaining if any such phenomenon were visible, as her apparently dense atmosphere might seem to render probable, but have never caught sight of anything which could be supposed to arise from that cause, except a very moderate elongation of the extremities of the crescent.

"*Wateringbury, April* 30, 1855."

[At the time when Mr. Guthrie's Note was communicated to the Society the Editor endeavoured to obtain some precise details respecting the phenomenon to which it refers, but it was found that Mr. Guthrie did not possess any written record of his observations. He merely remembered that they were made on the occasion of the inferior conjunction of the planet in December 1842. This was not, indeed, explicitly stated by him, but was inferred from the terms of his letter.]

---

*Extract from a Paper by Eyre B. Powell, Esq. on the Orbit of α Centauri.** (See *Monthly Notices* for January last, p. 87.)

"I have seen no other orbit of *α Centauri* than the one calculated by Captain Jacob, and appended to the Poonah Catalogue of Double Stars: between his and the present there is a great resemblance as to all the elements, save γ, Ω, and a, but in these we differ *in toto*. A table of variations, which I computed, explains our discrepancies: in the angles of position, like variations of γ and Ω, produce opposite changes, such that, from 1826 to 1848, a decrease of the inclination can be to a very great extent compensated by a retrogradation of the line of nodes; in the distances also there is a similar adjustability among different elements, the variations of a, γ, and Ω, playing the most prominent parts. The evident proximity of the primary star to the following side of the projected ellipse, and the necessary area of the perspective orbit, seem to mark that the line of nodes must lie nearly in the meridian, and if so, γ must possess a large value. With my second set of elements the area of the projected ellipse

* These remarks would have been inserted in the *Monthly Notices* for April, along with those of Captain Jacob on the same subject, had not the Paper which contained them been at the time in the hands of the printer for publication in the volume of Memoirs.—Ed.

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comes out 154 square seconds, and the sector between Captain Jacob's observations of 1846 and 1854, taken in connexion with a period of 75·3 years, gives an area of 153·6 square seconds for the ellipse. The elements also represent the observation of Fallows, as to position, with an error of only $+44'$, and that of La Caille, as to distance, with the trifling error of $-''.34$."

Notes on the Management of Chronometers and the Measurement of Meridian Distances. By Captain Charles F. A. Shadwell, R.N. C.B. &c. London, 8vo. 1855.

The author states that this work is mainly designed for the use of officers of the Royal Navy, but he expresses a hope that it may be perused with advantage by intelligent individuals of the Mercantile Marine. His object has been not so much to show how Chronometers may be rendered subservient to the ordinary purposes of Navigation as to exhibit their application to the advancement of Maritime Geography.

The work is divided into nine chapters, the first three of which are devoted to various preliminary details relative to the management of chronometers and to the determination of local time. In the fourth chapter the author explains the mode of ascertaining the rates of chronometers by two observations of a similar kind taken at a convenient interval. On this part of his subject he remarks:—"For the determination of the error on local mean time, it has formerly generally been the practice in the measurement of meridian distances, or on taking a departure from a port, to adopt as the starting-point the error shown by the last observation used in the determination of the rate; but inasmuch as in the deduction of the latter, we necessarily place equal confidence in both the observations, there seems to be no good reason why we should not do the same in assuming the error and adapting as our *working* error the mean of the two errors on which we have already agreed to make the rate depend.

"The *mean* error thus introduced may probably be assumed to be more accurate than either of the single elements on which it depends; while this mode of proceeding, moreover, will have the advantage of referring both the error and the rate to the same epoch."

The author remarks that this mode of viewing the subject has the advantage of simplifying the process for finding the corrections to be applied for the changes of rate which may have taken place in the transit from port to port.

The principles explained in this chapter are illustrated by a copious collection of examples. In one of these the error and rate of the chronometer are determined from observations of a *dissimilar* kind made in two places whose *difference of meridian is known*. "This example," says the author, "affords an illustration of an important remark of Raper's, 'that as the longitudes of the

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several places approach to precision, ships will employ the difference of longitude as a means of obtaining directly the *sea-rates* of their chronometers, instead of waiting to obtain harbour-rates; thus exemplifying one of the most important ends to which the perfection of hydrography can serve.'"

Upon the question with respect to the best interval for determining the rates of chronometers, the author justly remarks that if the stability of the rate could be relied upon, its value would be obtained with greater accuracy the longer the interval of time included between the observations for ascertaining the values of the error on mean time.

"In practice, however, this theoretic view is limited in its application by the impossibility of depending confidently on the steadiness of the rate over long periods, and by the consequent necessity for checking the performances of chronometers by frequent determinations of their errors, and thus breaking up the intervals on which the rates depend into short periods.

"As a matter of practice, therefore, it seems advisable when circumstances permit, that the rate of a chronometer should not depend on observations made at an interval of *less* than *five* or *more* than *ten* days. Seven days will be found a convenient average interval; and in the case of eight-day chronometers, moreover, it embraces the period affected by the whole weight of the chain.

"With the above limitations it may be laid down as a maxim that chronometers cannot be rated too often when time and opportunity permit.

"It seems advisable, moreover, when the measurement of meridian distances is in contemplation, that in so far as may be practicable, the two rates employed should depend on observations made at *equal* intervals of time; since, when the intervals are very unequal, the small errors of observation do not exercise an equal influence on the final results, and their values are unduly affected by the errors of observation attendant on the rate determined at the shorter of the two periods."

In the fifth chapter the author explains the mode of obtaining the errors and rates of chronometers by a combination of several observations taken within a convenient interval of time. The accumulations of the rate for the partial intervals form the groundwork of a series of equations of condition, which are solved by the method of least squares. On the question with respect to the advantage attending the application of this method, the author remarks, that "although in most cases the mean arithmetic error corresponding to the mean of the times of observation may be considered as sufficiently accurate and convenient for practice; yet in cases where an examination of the observations seems to indicate considerable instability or fluctuation of rate during the period of rating, and when the computer does not object to the additional labour involved in the latter more elaborate process, there is no doubt its results will usually be more satisfactory, and

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certainly more correct, while it will fully repay the extra trouble employed in its manipulation."

In the sixth chapter the author treats of the chronometric determination of meridian distances, explaining the different hypotheses which have been adopted for finding the variation of the rate during the voyage between the two places where the rate has been ascertained. The following chapter contains a copious collection of examples illustrative of the principles laid down in this chapter.

In the eighth chapter, the author shows how the difference of longitude between two places may be obtained from observations giving the *errors* of the chronometers at the two stations, independently of a knowledge of their preceding or subsequent *rates*. The *error* of the chronometer is ascertained: first, upon starting from the first station; secondly, upon arriving at the second station; thirdly, upon quitting the second station; and, lastly, upon arriving again at the first station. From these data the difference of longitude between the two stations, and the rate of the chronometer for the interval included between the departure from and subsequent arrival at the first station, termed the *travelling rate*, is deduced by a formula for which the author expresses his obligation to the Rev. George Fisher.

The ninth chapter of the work is devoted to a series of precepts on the mode of recording the results of chronometric measurements.

It may be remarked, in conclusion, that the subject-matter of the work is arranged in a very lucid order, that the explanations are concise and simple, and that they are in every instance illustrated by numerous examples. Possessing such claims to consideration, the work cannot fail to prove a most valuable manual to the naval profession in general, and more especially to the class of persons whose instruction the author had mainly in view while engaged in preparing it for publication.

Captain Shea has forwarded for inspection a volume containing the record of his observations of the solar spots. The appearances and disappearances to which he refers seem to be no other than the ordinary phenomena depending on the rotation of the sun upon its axis.

An azimuth compass for determining the variation was exhibited by Baron Kleinsorgen. It is assumed that the time of noon is known or may be determined on board, and consequently the direction of the true meridian at that moment. A line is stretched across the compass-box, and this box is turned until the shadow of the line coincides with the diameter of a fixed plane a little distance below it. The pin on which the card turns is

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connected with this plane, and there is an index corresponding with the diameter which points out the reading of the compass for the time of observation. Possibly in those merchant-ships where the altitude of the sun at noon is the only observation made, such an instrument might give useful warning to the helmsman, but the method only admits of a loose approximation.

New Variable Star.

Professor Argelander, in a letter to the Editor of the *Astronomische Nachrichten* (*Astron. Nach.*, No. 9580), announces the discovery of a new variable star, namely, the star W. VI. No. 1911 = Lal. No. 13825. It is represented by Lalande as an 8^m; by Bessel, once as an 8^m, and once as an 8.9^m. Fellöcker marks it in the Catalogue as an 8^m, but upon the Chart as an 8.9^m. Bremiker makes it a 9^m. However, on the 4th and 5th of March, 1854, and on the 4th of March, 1855, it was not visible in the comet-searcher, nor was it seen in the heliometer on the 30th of the same month; but it must be admitted that on this last occasion the sky was unfavourable for observation. Two days afterwards, however, it appeared like a faint star of the 9.10^m; and on the 11th and 12th of April it shone like a faint 9^m. It is of a very ruddy colour, and, like many other variable stars of small magnitude, appears to have a period of rather more than a year. Professor Argelander proposes to call it *R Canis Minoris*, and expresses his intention of following it as long as possible.

On several Stars which have Disappeared from his Ecliptical Charts. By M. Chacornac.

"On the 7th of August, 1852, at 15^h, I placed upon one of my charts, in $21^h\ 36^m{\cdot}5$ of right ascension and $+\ 14^\circ\ 33'{\cdot}9$ of declination, a star of the seventh magnitude, which was situate between two stars of the ninth magnitude. The position of this star was obtained by comparisons made with another star of the eighth magnitude in its vicinity.

"Next day I only compared with the heavens the regions of the chart where small stars were found to be situate; unfortunately, the region in which the star of the seventh magnitude had been placed was almost entirely wanting in small stars. I did not, therefore, verify it on that day, expecting on a future occasion to fill it up with small stars, and to re-observe at the same time the star which I had placed there.

"On the 20th of the same month, having finished this chart, I was very much surprised not to find again the star of the seventh magnitude, while those of the ninth magnitude were found to be perfectly in the place which my chart assigned to

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them. Convinced by this circumstance, and by the verification of the numerical results obtained on the 7th of August, that there could be no mistake, I undertook forthwith the search of this star upon the supposition that it was a planet, describing its arc of retrogradation. With this object in view, I constructed a rough draught of the neighbouring stars as far as the ninth magnitude. On the 30th of August this chart already comprised in the direction of the retrogradation 14° of right ascension, and extended on each side of the ecliptic 8° to 10° of latitude, without obtaining any result. I then learned that Mr. Hind had just discovered the planet *Melpomene* in the very region which I was exploring. This planet being fainter than the stars with which I was occupied, I immediately abandoned the search of the star of the seventh magnitude, which I supposed to be variable, in order to resume the construction of my charts. Since that time the star has not reappeared.

"On the 30th of December, 1852, I placed upon my charts, in $8^h\ 47^m{\cdot}3$ of right ascension and $+17°\ 44'$ of declination, a star of the ninth magnitude. I verified the position of this star only twelve months afterwards; that is to say, on the 4th of December, 1853. It was then invisible, and has not since reappeared. This star was near another very red star of the sixth magnitude.

"On the 5th of July, 1853, I also placed on a rough draught, in $16^h\ 8^m{\cdot}8$ of right ascension and $-22°\ 21'$ of declination, a star of the ninth magnitude near a nebula; these stars were not verified in the same year.

"On the 19th of May, 1854, upon completing this chart, I noticed that this star of the ninth magnitude had disappeared from the place where it was observed. It remained invisible till the month of August of the same year. Being sought for again on the 4th of April, 1855, it was found to be of the tenth magnitude.

"On the 8th of April, 1853, I observed a star of the eleventh magnitude in $11^h\ 3^m{\cdot}3$ of right ascension and $+6°\ 54'$ of declination. This star had precisely the same right ascension as another star of the ninth magnitude which was very near to it. On the 15th of the same month it was invisible. Having been again searched for on the 16th and 17th as a planet, I was not able to find it, either on account of the brightness of the moon or in consequence of its inconsiderable motion. On the 18th I knew that it was the planet *Themis* which M. De Gasparis had just discovered.

"On the 27th of December, 1853, I observed between two stars, very close to each other, between the eighth and ninth magnitudes, a star of the tenth, which I had not hitherto noticed. This chart not being filled up with small stars, I did not attach any other importance to the observation than that suggested by a multitude of new stars which are encountered in observations of this kind. I, therefore, placed this star of the tenth magnitude upon my chart, writing beside it at full length *triple star*.

"This triple star was only verified on the 26th of March, 1854.

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It was then only double, the small star of the tenth magnitude having disappeared. I examined this double star with a high magnifying power, and I again searched for the star which had disappeared upon the supposition of its being a planet, but without result. The following is the position of the double star:—$4^h\ 14^m{\cdot}6$ of right ascension $+23°\ 58'$ of declination.

"On the 30th of December, 1853, I observed a star of the eleventh magnitude in $3^h\ 33^m{\cdot}7$ of right ascension and $+20°\ 51'$ of declination. The unfavourable state of the weather prevented me from verifying its position with certainty till the 20th of January, 1854; it was then invisible. I have searched for it again as a planet, but without success. It has not reappeared.

"On the 10th of January, 1854, I found that a star of the eleventh magnitude, which must have been placed upon my charts between the 4th of September and the 29th of November, 1853, in $4^h\ 26^m{\cdot}9$ of right ascension and $+21°\ 26'{\cdot}8$ of declination, had disappeared. I searched for it again in my charts as a planet without result. It has not reappeared.

"On the 26th of January, 1854, I undertook the construction of a rough draught, upon which I placed very few stars during the evenings of the 30th and 31st of January. I did not resume the construction of this chart till the 30th of July of the same year: two stars had then disappeared from it. The one of the eleventh magnitude was not reobserved till later with the great refractor of the Observatory of Paris; it was then only of the thirteenth magnitude. The other of the eleventh magnitude has not been reobserved. The latter was situated in $23^h\ 27^m{\cdot}5$ of right ascension and $-4°\ 15'$ of declination.

"On the 19th of July a star of the tenth magnitude was placed upon my charts in $21^h\ 7^m$ of right ascension and $-15°\ 5'$ of declination. It was the planet *Urania* found by Mr. Hind. On the same day I found that a star of the ninth magnitude which must have been placed upon the chart in the course of the month of January 1854, in $21^h\ 20^m{\cdot}2$ of right ascension and $-12°\ 53'$ of declination, had disappeared.

"This star, which had been observed during the twilight, formed with other stars of the ninth magnitude a remarkable configuration. It was a little more brilliant than another star also of the ninth magnitude, which was very near to it.

"On the 11th of January, 1855, I found that a star of the eleventh magnitude, which had been placed on the 26th of October, 1854, upon one of my charts in $7^h\ 30^m{\cdot}3$ of right ascension and $+23°\ 54'{\cdot}7$ of declination, had reappeared. I was not able to reobserve this star earlier on account of the unfavourable weather which prevailed at Paris during the whole season of autumn. It has not reappeared.

"On the 28th of October, 1854, I found that a star of the eleventh magnitude, placed upon my charts in $2^h\ 47^m$ of right ascension and $+17°\ 32'{\cdot}5$ of declination, had disappeared on the 26th of September in the same year.

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"It was a position of the planet *Pomona*, which M. Goldschmidt had just discovered.

"In the ninth hour, and quite recently, two stars have also disappeared from the positions which my charts assigned to them.

"One of the tenth magnitude which was placed upon these charts towards the end of the year 1854, has been invisible since the 17th of January, 1855. The other, which was of the eleventh magnitude, was observed on the 25th of January of the same year, and placed upon my charts in $9^h\ 27^m{\cdot}2$ of right ascension and $+16^\circ\ 8'{\cdot}7$ of declination. It had disappeared on the 19th of March, at which date only I was enabled to verify its position.

"These two stars have been searched for again as planets and variable stars, but without result.

"On the 5th of October, 1853, I perceived a star of the twelfth magnitude in a region wherein my chart did not exhibit any. This chart was, however, pretty well filled up. I affixed a mark to this star and proceeded on the following day to verify its position. Clouds permitted only a short verification, during which I found that the star had disappeared from the place where I had observed it on the preceding evening, but I was unable to find it again in the neighbouring region. I searched for it again on the following days with great care, but without success. Since then it has not been found in the position of the 5th of October, which was this,—

$$\text{R.A.} = 0^h\ 44^m{\cdot}4 \qquad \text{Decl.} = +8^\circ\ 46'{\cdot}2$$

"In order that it may be understood how I have allowed so many stars to escape from me, I ought to mention that, in order to construct my ecliptic zones with rapidity,—although I always had the intention of verifying on the following evening the position of the stars observed on any preceding evening,—I have frequently been compelled by the unfavourable change in the weather to defer the verifications to other epochs, and to profit by the beautiful nights in occupying myself especially with the regions of the ecliptic in opposition to the sun. I ought also to mention that it has happened to me, at Paris especially, to have been obliged to postpone for several months similar verifications by the coincidence of the rare instances of a clear sky with the full moon; for it is known that when the question relates to the small planets, the great brightness which the moon diffuses over the entire heavens, when it is approaching opposition, effaces their light as effectually as the close proximity of that body. The usually not very serene condition of the sky at Paris, the inconsiderable altitude of these stars above the horizon, are so many causes which prevent the searching for and verifying of them as often as is possible in the case of stars of a higher magnitude.

"*Imperial Observatory, Paris, April* 15, 1855."

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Astronomical and Meteorological Observations made at the Radcliffe Observatory in the Year 1853. Under the Superintendence of Manuel J. Johnson, M.A., Radcliffe Observer. Vol. XIV. 8vo. Oxford, 1855.

The principal part of this volume is devoted to the class of observations required for the construction of the Circumpolar Catalogue. An appendix to the volume contains the observations with the heliometer. In the introduction to this part of the work Mr. Johnson gives the details of his investigation of the parallaxes of 61 *Cygni* and 1830 *Groombridge*, to which allusion was made in the Annual Report of the Council for this year. With respect to the observations with the heliometer, a few consist of observations of the diameters of the planets; but they chiefly refer to double stars, and to measurements of the relative positions of a few select stars for parallax. The latter class of stars includes *Arcturus* and *α Lyræ*.

New Comet.

A comet was discovered by M. Klinkerfues at Göttingen on the 4th instant. The following position of it was obtained by M. Rümker at Hamburgh on the next evening:—

	Hamburgh M.T.	Comet's R.A.	Comet's Decl.
	h m s	° ′ ″	° ′ ″
1855, June 5	10 49 37·0	107 50 58·7	+36 15 53·5

Instruments for Sale, formerly belonging to the late Bryan Donkin, Esq.

A 3½ in. achromatic telescope and claw-stand, by Tulley	£35 0 0
A position micrometer, by Troughton	4 0 0
A mountain barometer, by Troughton	7 0 0
Ditto, by Bautern	3 0 0
A 30-in. transit and stand collimator, and a Donkin level for ditto	15 0 0
A sextant and stand, with bottle, &c., by Troughton ..	14 0 0
A 12-in. theodolite, plates and legs, by Troughton ..	10 0 0
A Y level, 30 inches, by Simms	6 0 0
A level, 20 inches and legs	5 0 0
A dynameter, by Dollond	2 0 0
Two 2-feet pentagraphs, one by Troughton	3 0 0
A small solar microscope	1 0 0
A perspective machine	1 0 0
An equatoreal block	1 0 0
A grinding tool, by Holtzapffel	1 0 0
A hygrometer, by Daniel	1 0 0

They may be seen at the rooms of the Royal Astronomical Society.

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Instrument for Sale.

Object-glass, by Lerebours and Secretan of Paris; focal length, 86·3 in.; aperture, 6·2 in.; not quite perfect, having striæ at two points of the circumference. The central four inches are very good. The measures of 144 double stars given in the last volume of *Madras Observations* were taken with this glass. Price 28*l.* Inquire at the apartments of the Society.

A Lassell's machine for grinding and polishing specula for reflecting telescopes, made by Mr. Nasmyth of Patricroft. Price 30*l.* Inquire at the Society's apartments.

ERRATA.

The upper line of the figure X, referred to by Captain Jacob in his Paper on the Orbit of *α Centauri*, inserted in the *Monthly Notices* for April (p. 179), instead of being *rectilinear*, as incorrectly drawn by the engraver, should have been *slightly curvilinear*, so as to adapt itself to the periphery of the ellipse.

Page 181, line 4 from bottom, *for* 52° 24′ 52″, *read* 52° 54′ 52″.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *June* 8, 1855. No. 8.

M. J. JOHNSON, Esq. President, in the Chair.

Lieut. Tennant, Bengal Engineers, India;
Rev. S. Newth, New College, London; and
Capt. W. Noble, R.A., Woolwich,

were balloted for and duly elected Fellows of the Society.

On the Computation of Double Star Orbits. By Capt. W. S. Jacob, East India Company's Astronomer.

"In bringing this subject to the notice of the Society, it is proper to premise that I have no decidedly new method to propose; but it has occurred to me, having computed many orbits in various ways, that my experience may be of some service to others, who may feel disposed to take up the subject, by suggesting certain facilities in minor matters of detail, by which time may be saved or greater accuracy attained.

"The plan I follow is, in the main, Herschel's first method given in vol. v. of the Society's *Memoirs;* his second, or improved method, in vol. xviii. I have also tried; but, though more elegant and scientific in form, it does not appear to me to insure greater accuracy, while it has the disadvantage of working *in the dark*, instead of allowing scope for the judgment to be exercised at every step, which, considering the loose character of much of the materials to be dealt with, appears to me of great importance. In order that this second method may be advantageously applied, the observations would have to be all of about the same order of goodness, and pretty equally distributed round a considerable part of the periphery of the ellipse,—conditions which are scarcely as yet attainable in a single instance; and it would also be essential that all the computations should be made in duplicate and compared at every step, since a small numerical error would vitiate the whole results, and would be by no means easy of detection.

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"I have, therefore, after a few trials, abandoned this method, and returned to the old or graphical mode with very slight alterations. In the preliminary operations up to the first approximation to the apparent orbit, Herschel's rules are exactly followed; occasionally, when the distance-measures have been sufficiently good and numerous, I have projected them, as well as the angles, into a curve, and have brought the two curves into unison by slight alterations and successive trials; but the occasion for this has rarely offered, and it is in general easy to introduce the effect of the distances in the final corrections. Having then laid down the corrected places from the position curve, I cut out in paper an ellipse of about the proper size, and adapt this so as to agree as nearly as possible with the points laid down; it is then easy to see, not only the proper direction of the major axis, but also whether any variation is required in the dimensions of the ellipse: having thus got approximately the apparent orbit, the real elements are obtained by Herschel's rules, with the following exceptions: having found a' and α graphically, I find it lead to greater accuracy to compute b' and β by the following formulæ rather than to get *them* also by the graphical mode:—

$$\tan B = \frac{t^2}{c^2} \tan A$$

$$a' = \frac{t \cdot \sec A}{\sqrt{1 + \frac{t^2}{c^2} \tan^2 A}}$$

$$\frac{a'}{b'} = \frac{t \cdot \sec A}{c \cdot \sec B}$$

where t and c are the transverse and conjugate axes of the apparent ellipse, a' and b' the projected major and minor axes of the true orbit, A the inclination of a' to t, and B that of b' to c.

"To obtain the mean motion and epoch, Herschel proposes two methods: first, to cut out of card and weigh in a nice balance the whole ellipse and the portions included between the extreme dates of observation and the projected major axis; or second, analytical, by the resolution of certain equations: the first appears to have been the plan generally preferred; the second having been probably rejected by common consent as needlessly laborious. I have not followed either, not having, in general, had at hand a balance accurate enough for the purpose, and being also doubtful of the advantage of the statical mode over that of simple mensuration:—whether, even supposing the weights could be obtained with perfect accuracy, it would be possible to cut out the ellipse more exactly, or even so exactly as it could be measured. My plan is to divide the portion of the ellipse included between the observations into triangles and segments, and measure them in the ordinary way: the area of the elliptic segment being approximately $= 0{\cdot}67\, ch$, where c is the chord and h the height or versed sine of the segment; this is exact for the parabola, and

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practically so for the ellipse when the segment is small enough not to differ sensibly from that of a parabola,—say, in general, when c exceeds $10h$. Supposing the observations spread over a large portion of the ellipse, so as to include within them (or nearly so) the area of one or more quadrants, it will evidently be sufficient to measure the *difference* of the total area described from that of the included quadrants.

"For drawing ellipses I have found the best mode, both as regards convenience and accuracy, to be that of co-ordinates, depending on the fact that all ellipses with a common axis (major or minor) have the ordinates on the same abscisses proportional to the *other* axis; consequently, since the circle is one of them, if the abscis* $x = a \sin X$, then $y = b \cos X$.

"There are several of these co-ordinates which can be easily retained in the memory, so that no time need be lost in referring to tables; thus,—

If $x = a \times$ ·28	then $y = b \times$ ·96
·50	·866
·60	·80
·707	·707
·80	·60
·866	·50
·96	·28

"It will be observed that there are only 4 ratios to be remembered, and they furnish 7 points in each quadrant, besides the extremities of the axes, or 32 points in the whole periphery, through which the curve may be readily drawn with a curve-ruler, such as is used by architects for tracing their mouldings.

"For approximately solving the equation $u - e \cdot \sin u = m$, Sir J. Herschel mentions having constructed a machine of wheel-work: this is, doubtless, a very ingenious contrivance, and probably useful for other purposes; but it is not at all essential here, since a solution may be obtained by the common slide-rule. The line of sines on a Bate's 10-in. rule will give the value by simple inspection to about $0^{\circ}{\cdot}1$, or even less; whereas the machine was said to give it only to the nearest degree.

"There is nothing further to be noticed till we come to the final corrections. After comparing the computed with the observed places, I formerly used to correct the elements by the method of least squares. This proved unsatisfactory; the labour was great, and the result by no means commensurate, since the changes required in the elements were often so large as to render sensible the effect of variations of the second and higher orders, and it was not easy to introduce these into the equations of condition; and I now adopt a partly graphical mode, by laying down carefully on a large scale the apparent orbit from the computed places. This will not agree exactly with the one first assumed,

* x having its origin at the centre.

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because slight and obvious corrections will probably have been introduced into the elements (especially Ω) in the course of computation: the eye will then readily detect the alterations required in the course or dimensions of the curve to make it agree more closely with observation, and it will sometimes be an assistance to cut out in paper an ellipse of about the right size, and see whether by turning it this way or that the proper amount of deviation can be given. In this way, after a few trials, a result will be obtained as good as the materials are capable of giving.

"It will generally be advisable to compute the coefficients of variation for the different elements, to serve as guides in making these final alterations. They are very easily computed as follows:—

$$\frac{\Delta\theta}{\Delta\tau} = \text{annual motion (apparent)}$$

$$\frac{\Delta\theta}{\Delta n} = \text{annual motion} \times \frac{t-\tau}{n}$$

$$\frac{\Delta\theta}{\Delta e} = \frac{\text{annual motion}}{n} \times \sin u \left(1 + \frac{\sin u}{2 \sin v \,.\, \sqrt{1-e^2}}\right)$$

$$\frac{\Delta\theta}{\Delta(\log\cos\gamma)} = \frac{1}{\text{tab. diff. log tan } (\theta - \Omega)}$$

$$\frac{\Delta\theta}{\Delta\lambda} = \frac{\text{tab. diff. log tan } (v+\lambda)}{\text{tab. diff. log tan } (\theta-\Omega)}$$

"The annual motion is easily deduced from the computed change of angle for any given interval, and the mean distance for that interval, remembering that the rate of motion is always in the inverse ratio of the squares of the distances.

"The coefficients thus obtained may be employed, if it be thought fit, in forming equations of condition for solution by the method of least squares, but it will in general be found preferable to use them, in conjunction with the graphical mode above described, to correct the elements by trial and error; for having found graphically the general direction of the alterations required, the coefficients will enable us to judge on which of the elements the changes can most advantageously be thrown, so as to produce the greatest proportional effect on the resulting angles.

"The following elements for the orbit of *γ Coronæ Australis* were obtained in the manner above described, being the result of the second trial:—

τ =	1863^{yr}·08	
P =	100·80	
n =	3°·5715	
π =	256° 12′	
Ω =	352 13	
λ =	266 25	
γ =	53 35	l. cos = 9·7736
e =	0·602	
a =	2″·549	

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Comparison.

[Date.	l_o.	$l_c - l_o$. ∠	$l_c - l_o$. arc.	ϱ_o.	$\varrho_c - \varrho_o$.
	° ′			″	
1834·47	37 6	+46	·034		
1835·55	36 48	−48	·035		
1836·43	34 30	− 3	·002		
1837·43	32 42	+ 1	·001	2·66	−·14
1847·32	14 6	+ 5	·003	2·30	−·01
1850·46	5 52	+77	·047	2·29	−·20
1851·54	4 28	− 3	·002	2·26	−·20
1852·27	3 27	−59	·034	1·89	+·12
1852·72	0 58	+13	·007	1·91	+·04
1853·25	359 35	+ 3	·002	1·83	+·08
1853·78	358 30	−30	·016	1·82	+·05
1854·26	356 10	+16	·008	1·71	+·12

" The signs of the distance-errors would indicate some further slight correction of the elements, but the early distances are not worthy of much confidence.

" *London, 7th June*, 1855."

M. Drach has received a letter from Dr. Donati, of the Florence Observatory, informing him of his having discovered, on the 3d instant, a new comet in the constellation *Telescopium Herschelii*. With the ring micrometer, and a low magnifying power, and through mist and clouds, he obtained the following positions:—

	Florence M.T.	Comet — Star. In R.A.	In Decl.
1855.	h m s	m s	′ ″
June 3	10 4 10	−2 17·18	+22 0·0
4	9 55 12	+2 37·78	− 9 22·9
5	9 18 36	−4 2·41	−20 12·5

Comet's App. R.A.	App. Decl.	No. of Comparisons.
h m s	° ′ ″	
.....	.....	2 with (*a*)
6 56 56·27	+36 22 5·5	1 with (*b*)
7 10 32·73	+36 15 15·1	1 with (*c*)

Apparent Positions of the Comparison Stars.

	R.A.	Decl.	Catalogue of Stars.
	h m s	° ′ ″	
(*b*)	6 54 18·49	+36 31 28·4	Lalande 13569
(*c*)	7 14 35·14	+36 35 27·6	— 14298

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The star (*a*) is of 10.11 magnitude. Clouds prevented its being observed after the evening of the 3d. The comet does not exhibit any trace of either tail or nucleus. It appeared somewhat fainter than the nebula in *Hercules*.

[This comet is the same as that mentioned in the *Monthly Notices* for May as having been discovered by M. Klinkerfues at Göttingen on the 4th of June. Dr. Donati's discovery is, consequently, a day earlier than that of the latter.—EDITOR.]

On the Values of the Constants of Nutation and Aberration, and of the Parallax of γ Draconis, as Deduced from the Observations made with the Twenty-five Foot Zenith Tube, at the Royal Observatory, Greenwich. By the Rev. R. Main.

The observations discussed in this Paper extend from June 1837 to May 1848, during which time they were made with tolerable continuity, the only interruptions being such as were inevitable either on account of periods of bad weather, or from the difficulty of seeing the star in the day-time, or from occasional readjustments or alterations in the instrument.

The author commences with a preliminary recapitulation of the principal results obtained by preceding astronomers for the values of aberration and nutation. He next gives a description of the instrument employed in the observations which form the groundwork of his own investigation, referring for further details to the Introduction of the *Greenwich Observations* for 1837 and subsequent volumes.

The zenith tube was erected by Troughton during the Directorship of Pond for the purpose of observing several stars in addition to *Polaris*, which pass the meridian of Greenwich very near the zenith, but no regular series of observations suitable for any delicate inquiry was made with it till the accession of Mr. Airy. One of the first points taken into consideration by the latter astronomer was the examination and improvement of this instrument, with the view of rendering it efficient for making an extensive series of observations of *γ Draconis*.

It may be mentioned, for the sake of showing the fineness of the micrometer-screw, that the value of a revolution was found from the mean of several determinations made in the winter of 1836 and agreeing very closely with each other to be $6''.714$; a similar determination in the spring of 1846 gave $6''.724$ for the value of a revolution.

After describing the various precautions which were taken to prevent any disturbing influence from vitiating the observations, the author next proceeds to explain the processes pursued in the calculations.

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First, the observations were divided into convenient groups, rarely extending over an interval of time greater than fourteen days; and it is assumed that for these intervals the mean of the observed apparent zenith distances corresponds accurately to the mean of the days of observation. This supposition is sufficiently accurate even with reference to the changes of the coefficients of aberration and parallax.

The author thus obtains a series of normal apparent zenith distances which theoretically ought to be very accurate, and each of which may be represented by an expression containing the following terms:—

1. The assumed mean north zenith distance for an assumed epoch.
2. The correction due for precession and proper motion for the interval of time comprised between the epoch and the day corresponding to the mean of each group of observed zenith distances.
3. The correction for aberration.
4. The correction for parallax.
5. The correction for nutation.

The assumed epoch is 1840, January 1, and the assumed north zenith distance for that epoch is $118''{\cdot}00 + v$, the latter quantity representing a correction, the value of which is to be determined from the investigation.

For the precession the assumed value of n is that given by M. Peters in his *Numerus Constans Nutationis* for 1840, namely, $20''{\cdot}057$; and the assumed proper motion is that deduced by the author himself in his paper on the "Proper Motion of the Fixed Stars." (*Mem. R. Ast. Soc.*, vol. xix.) These, when combined, give $-0''{\cdot}66$ for the annual variation in north zenith distance. For convenience of application a table is made of the amount of this correction for every hundred days.

The constant of aberration is assumed to be $20'' + x$, and the constant of parallax is represented by y, x and y being unknown corrections.

The constant for nutation is assumed to be for 1840: $9''{\cdot}223\left(1 + \frac{z}{10}\right)$, z being an unknown quantity, which, in like manner as v, x, y, is to be determined from the investigation.

The mean zenith distance corresponding to a group after receiving the various corrections above-mentioned computed for the mean date of the group, is compared with the observed zenith distance, and an equation of condition is thus formed, involving besides the three unknown quantities, x, y, z, a constant and unknown correction to the mean zenith distance denoted by v. A comparison of the computed with the observed normal apparent zenith distance for each group furnishes one of such equations, and thus a series of equations of condition are obtained, from which the values of the unknown quantities v, x, y, z, are to be determined.

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The total number of equations of condition is 161. Treating them by the method of least squares, the author obtains the following results:—

$v = -0''.173$; weight 528·007; prob. error 0·047
$x = +0''.058$; weight 240·192; prob. error 0·071
$y = -0''.378$; weight 266·231; prob. error 0·067
$z = +0''.118$; weight 289·281; prob. error 0·064

Hence, as the assumed mean distance for 1840, January 1, was $118''.00$; the constant of aberration $20'' + x$; and the constant of nutation for 1840, $9''.224\left(1+\frac{z}{10}\right)$, we have finally,—

Mean Zenith Distance North of γ *Draconis* for 1840, January 1	$= 117''.827 \pm 0''.047$
Constant of Aberration	$20''.058 \pm 0''.071$
Constant of Lunar Nutation.....	$9''.335 \pm 0''.067$
Constant of Parallax	$-0''.378 \pm 0''.067$

The author concludes with the following remarks:—

"The anomalous results here indicated for the aberration and the parallax seem to show that some cause of error of a periodical character has acted injuriously on the observations; but it is very difficult to imagine a source of error of this nature. A want of fixity of the object-glass, or of the grand micrometer, or of the plumb-line microscopes, would have exhibited itself in producing inconstancy in the zero, and sudden irregularities in the observations; but, on the whole, the zero has, through the whole series of the observations, all desirable steadiness, and the observations are as consistent with each other as could be expected. The value of a revolution of the grand micrometer was determined by an exceedingly accurate method, twice in the interval of a few years, and the two results were almost identical. One determination was made in the winter of 1836, and the other in the spring of 1846; and it is probable that the average temperature was pretty nearly the same at each determination; but, even if the result be affected with an unknown temperature correction, the range of arc required for observations of γ *Draconis* is so small, that no significant error is to be apprehended on this account. The only remaining supposition is that of some derangement or deflection of the plumb-line, and the chance of any disturbance of such magnitude and constancy as the case requires having happened, appears to be very small. If the plumb-line should have been pulled out of the vertical by means of the auxiliary apparatus added in 1840 for the prevention of the twist caused by rapid rotation of the instrument, the deviation would be totally in the prime vertical, and the projection on the meridian plane would still be accurately vertical. It may be added also, that any deviation of the plumb-line from the vertical would be eliminated by the double observation in reversed positions.

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"Still, it is believed that, notwithstanding the doubt which, to a certain extent, seems to rest on the results of the investigation, on account of the anomalous values deduced for the aberration and the parallax, considerable weight may still be attached to the value deduced for the nutation-constant, since, on the supposition of an unknown cause of error producing an inequality, having either an annual or a shorter period, its effect would be perfectly eliminated in so long a series of observations.

"The labour bestowed upon the investigation in the endeavour to produce the best possible result from the observations by applying rigorously the method of least squares, has been very great; and, as the whole of the calculations have been performed by myself, I regret much that the results have not proved so unexceptionable as to warrant the amount of care and labour expended. Still, a laborious work of this kind cannot be without its value; and it is hoped that additional scrutiny may afterwards throw some light upon the nature of the errors in the results of an investigation based upon observations made in an unexceptionable way, with an instrument faultless in theory, and pursued by processes combining all the modern refinements.

"For the present I leave this Paper in the hands of astronomers, with the hope that some one interested in the subject may be able to offer some elucidation of the difficulty thus presented.

"1855, *May* 30."

Results of Astronomical Observations made at the Observatory of the University, Durham, from October 1849 *to April* 1852, *under the general direction of the Rev. Temple Chevallier, B.D., F.R.A.S., Professor of Mathematics and Astronomy.* By R. C. Carrington, Esq., B.A., F.R.A.S., Observer in the University. 8vo. Durham, 1855.

In an Introduction, embracing sixteen pages, the author gives a description of the instruments and mode of observation pursued.

From October 1849 to the beginning of May 1850, the observations were almost exclusively meridional; thenceforward, with little exception, equatoreal. The objects observed in the meridian during 1849 and the spring of 1850 were, for the most part, stars from the Catalogue of the British Association, of which modern observations were wanted; the moon and moon-culminating stars, and the planets *Mars*, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*. After April 1850, the objects were mainly such of the comparison stars used for the equatoreal as were not too faint for the transit telescope. The objects observed with the equatoreal were the planets *Pallas*, *Juno*, *Hebe*, *Iris*, *Flora*, *Metis*, *Hygeia*, *Parthenope*, *Victoria*, *Egeria*, *Irene*, and *Eunomia*; and the comets called Petersen's third, Bond's, D'Arrest's, Brorsen's fourth, and Encke's.

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The equatoreal observations were published provisionally from time to time in the *Monthly Notices* of this Society, and in the *Astronomische Nachrichten*. Their appearance in a definitive form was, however, delayed, in consequence of the transit-circle of the Durham Observatory being too small for observing the greater number of the comparison stars, and the necessity which therefore existed for obtaining those data from some other sources. The author, before his connexion with the university had ceased, offered to provide for this want as far as he was able, and to superintend the future revision and publication of the observations in a final form, as soon as the star-places were supplied. For a part of these he was indebted to the Astronomer Royal and Professor Piazzi Smyth; the others, or at least those which did not require reobservation, were determined by himself at the observatory which he subsequently erected at Redhill. The total number of comparison stars is 221.

The instruments of the observatory consisted of a small transit-circle of $3\frac{1}{4}$ in. aperture, and 4 feet 2 in. focal length; an astronomical clock, by Hardy; an 8-feet equatoreal by Fraunhofer; and a second equatoreal of 7 feet, presented by the late Duke of Northumberland; besides some smaller instruments not in actual use.

The Fraunhofer refractor with which the equatoreal observations were made had an aperture of $6\frac{1}{2}$ in., and a focal length of 8 feet 3 in.

The first section of the work contains a catalogue of the mean places of stars deduced from observations made with the transit-circle. The second section exhibits the apparent positions of planets observed with the same instrument, and their comparison with the corresponding numbers given in the *Nautical Almanac*. The third section is devoted to observations of the moon and moon-culminating stars, also taken with the transit-circle. The fourth section contains the details of occultations of stars by the moon, observed with the Fraunhofer equatoreal, with the equations of condition deduced from them for the correction of the assumed elements computed according to the method of Professor Challis, given in the *Nautical Almanac* for 1854. The total number of occultations treated in this manner is sixteen. In the fifth section the author gives an account of the determination of the longitude of the observatory by the transmission of chronometers between Greenwich and Durham in the year 1851. The chronometers were conveyed twice to and from Greenwich by railway. The value of the longitude deduced from the first journey was $6^m\ 19^s{\cdot}79$ west; the corresponding result of the second journey was $6^m\ 19^s{\cdot}72$ west. The longitude definitively adopted was $6^m\ 19^s{\cdot}75$ west. From observations of the moon and moon-culminating stars, the author's predecessor had found the longitude to be $6^m\ 19^s{\cdot}1$ west.

The remaining sections are devoted to the equatoreal observations of the small planets and comets, and to the observations of the comparison stars used in conjunction with them. It is to this part of the work that the author more especially invites attention,

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under an impression that the final results may prove of some service in the correction of the orbits of the various bodies to which the observations relate.

On the Determination of the Difference of Longitude of the Royal Observatory, Greenwich, and the Harvard College Observatory, Cambridge, U.S.

(*Extracts of Letters from Mr. Bond of Harvard College Observatory, Cambridge, U.S., to Mr. Hartnup of the Liverpool Observatory.*)

"I have again to solicit your valuable assistance in the further prosecution of the expedition for determining the difference of longitude between this place and Greenwich. The results thus far obtained by means of chronometers prove so satisfactorily accordant that Dr. Bache, the Superintendent of the United States' Coast Survey, deems it expedient to continue our operations. With his permission I enclose you the results of the chronometer expeditions of 1849, 50, and 51, which have been recomputed by my son, Mr. G. P. Bond; the details are now nearly ready for publication, and will shortly be sent to press.

"The experience of former years has suggested some precautions and minute attentions, not heretofore, I believe, attended to in any similar expedition, which render it desirable to have your co-operation. If you feel disposed, and your other avocations will permit of an affirmative answer, it will afford me great pleasure.

"*March* 14, 1855."

"My son, Richard F. Bond, and Mr. Philip Sidney Coolidge, will go out to Liverpool on the first voyage, and we are very anxious to have everything previously arranged with the authorities of the Liverpool Custom-house, that there may be no delay in landing the chronometers at your observatory immediately on the arrival of the steamer in the Mersey. We have taken state-rooms in the steamer which is to sail from Boston on Wednesday, the 6th of June.

"A spring-governor, and electric clock for records, will accompany the chronometers; and it will be very desirable to have the temperature maintained as equable and as near that of the mean temperature to which they are exposed on the passage as possible. This being, probably, the last and a test expedition, we shall use every precaution to ensure correct data for the reductions.

"*May* 14, 1855."

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Report of the Astronomer Royal to the Board of Visitors, read at the Annual Visitation of the Royal Observatory, Greenwich, 1855, June 2.

The following are a few extracts from this Report:—

"The Twentieth of the Reports which I have had the honour to present to the Board of Visitors will, I trust, be received with indulgence, less on account of the importance of the history which it is to convey, as applying to a year in which no changes of great consequence have been made, than for the numerical place which it occupies in the series of Annual Documents. The date to which the statements of the Report apply is 1855, May 15; and the interval embraced in its historical portion is that between 1854, May 26, and 1855, May 15.

"*Astronomical Instruments.*—On the transit-circle, I have only to remark that it is in perfect order, and continues to give the greatest satisfaction to all the observers.

"The transit-circle at the observatory of the Cape of Good Hope (constructed generally on the pattern of that at Greenwich, with some improvements) is mounted, and is, I believe, in full work. It seems to be quite satisfactory to Mr. Maclear.

"The reflex zenith tube is general good order.

"No alteration has been made in the altazimuth.

"The barrel-apparatus, for the register of transits by punctures produced by galvanic communication, has been in constant use without suffering injury, except in the parts exposed to continual friction, which require occasional attention. The method of giving the time-second-signals from the transit-clock, which I I described in my last Report, is found to be perfectly successful. The insulation of the touch-apparatus has sometimes failed in very damp weather; but, when the sky has cleared, the moistened gutta-percha has become dry and the insulation perfect, so speedily that very few transits have been lost.

"The rest of the galvanic apparatus is, in most respects, in the same state as at the last meeting of the Visitors. In the galvanic magnet for dropping the time-signal-ball, it has been found desirable to guard against the risk of permanent magnetism, by causing the apparatus itself to reverse the poles of the battery at every drop of the ball. When arrangements were originally made for exhibiting the London currents upon the transit-clock needle, and for sending currents to and through London by the touch apparatus of the transit-circle, in order to avoid disturbing the ground, I so connected the wires by turn-plates that one of the wires of the barrel-apparatus was used for these purposes; but with the increased facilities which I now possess for laying wires, I intend to make the barrel-apparatus-wires entirely independent of the others,—preserving, however, the power of connecting the touch-apparatus with the London and foreign wires.

"*Astronomical Observations.*—The well-understood system

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of meridional observations of stars remains unaltered, each of the stars in the extended standard list being observed, if possible, twenty times in three years. The moon is observed on the meridian at every opportunity, without exception. In the observation of planets a slight relaxation has been made, which the increased number of those bodies rendered necessary. The small planets are not observed at all in the morning watch (from 3^h A.M. to daylight), and the large planets are not observed then except in company with the moon. With this qualification, the sun and planets are observed on the meridian at every opportunity, except on Sundays.

"The whole number of meridional observations from 1854, May 26, to 1855, May 15, is nearly as follows (an observation of two limbs, or a duplicate transit by eye and ear, and by touch-apparatus, being reckoned as two):—In the transit department; transits, 4680; observations of collimators by means of the transit-telescope, 302; observations of transit-wires by reflexion, 302; observations of one collimator by the other, 52: in the meridian-circle department; observations of all kinds, 4565.

"The number of days on which *γ Draconis* has been observed with the reflex zenith tube is 51.

"With the altazimuth, the number of days of complete observations of the moon during 12 lunations is 178, or 14·8 per lunation, while with the transit-circle the number has been 97, or 8·1 per lunation. The sky has evidently been less favourable than usual; the failing has taken place principally in the spring of the present year. Of the altazimuth observations, 0 are on days when the moon passed the meridian between 0^h and 1^h solar time, 2 between 1^h and 2^h, 4 between 2^h and 3^h, 3 between 21^h and 22^h, 2 between 22^h and 23^h, 2 between 23^h and 24^h. There are no observations on the meridian corresponding to these. The whole number of separate observations of the moon and stars with the altazimuth is 878, and the whole number of observations of its collimator is 604. The observations have been conducted exactly as in the last years.

"The following remarks on the result of the reductions may not be without interest:—

"During the whole time of which I have spoken, the galvanic-contact method has been employed for transits, with the exception of a few days, when the galvanic apparatus was out of order. From the clock-errors, I have deduced the personal equations of the observers in our usual way; not by making special experiments, in which I have very little confidence, but by taking the transits as we find them, and discussing them on the supposition that the clock-rate has been very steady; a supposition in the adoption of which we are amply justified by the comparison of the clock-errors. The result is, that the magnitude of the personal equations in the galvanic-touch method is not above half of that in the eye-and-ear method.

"In former communications to the Visitors, I have alluded to

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the constancy of relation between the azimuths of the transit-circle and of the collimators, while all seem to change with respect to the heavens. Other instances of the same thing have occurred from time to time, and have left upon the minds of the computers the impression that there is a real change in the position of the ground. I need not remark that such a conclusion can be received as valid only after very careful and complete discussion.

"*Chronometers, Communication of Time, and Operations for Longitude.*—The number of chronometers now on hand is about sixty; twenty of these being on trial for purchase. They are compared, some every day and some every week, and occasionally in extreme temperatures; the repairs of those which belong to the Government are managed; and weekly reports of rates, and monthly reports of repairs, are made to the Admiralty.

"The system of galvanic normal clock and sympathetic clocks is in the state described in the last year's Report; with this difference only, that the wires to the clock at the Hospital Schools, instead of being suspended across the Park, are carried underground. The clock at the London Bridge Station is made to distribute the galvanic hourly signals to the Electric Telegraph Company's wires and to the different branches of the South-Eastern Railway wires.

"The time-signal ball at Deal was brought into regular use at the beginning of the present year. In a short time, however, its action was interrupted, partly by derangement of the apparatus, and partly by the severity of the weather, which froze the sulphuric acid to the state of jelly. I sent an assistant and workman to put it in order, and since that time it has generally acted very well. Since March 2 there have been three failures; one of these arose from the ball hanging on the clips, which were not properly oiled, and one from the turning off of the current on the Railway line; the cause of the third has not been traced out with certainty. The success or failure of the drop is known immediately at Greenwich; as the Deal ball, at the termination of its fall, so alters the connexions of wires that a signal is sent to the Observatory. A register is kept in a prescribed form in the Ball-Tower at Deal, and copies of this are sent to the Observatory, leaf by leaf, as soon as they are filled. These cautions I consider to be absolutely necessary for maintaining the regular action of the mechanism under rather difficult circumstances. The whole system is so successful that I have no hesitation in recommending its extension to the Government.

"Application has been made to me from one of the important offices of Government, for the galvanic regulation of their clocks. On considering the risks to which various galvanic communications are liable, and the financial necessity for occupying wires as little as possible, I perceived that it was necessary to devise constructions which should satisfy the following conditions. First, that a current sent once a-day should suffice for adjusting the clock, even if it had gone ten or more seconds wrong. Secondly,

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that an occasional failure of the current should not stop the clock. I have arranged constructions which possess these characters, and the artist (Mr. C. Shepherd) is now engaged in preparing estimates of the expense. I think it likely that this may prove to be the beginning of a very extensive system of clock regulation.

"I have commenced examination of observations and preparation of skeleton forms for the extension of lunar reductions.

"*General Remarks.*—A fair examination of the statements already made will show how much our real disposable force has been weakened, by accidental circumstances, during the last year. Two Assistants were absent for a month on the Pendulum expedition, and the whole of the extensive calculations which followed were made in the Observatory. One Assistant was employed for some time at Deal, another being at the same time partially occupied in London. The longitude of Paris required the absence of an Assistant for some weeks, and threw a great mass of calculation upon us. For mounting the model of the transit-circle at the Exhibition in Paris, one Assistant has been employed in Paris (with four workmen) for a month. It is under these pressures that I have been obliged to require the assistance of supernumeraries for observations—an assistance which I have been compelled to purchase by the sacrifice of part of their time, otherwise available for computation.

"At the same time, I view the progress of our ordinary work cheerfully. We have effectually maintained the mastery over it; and, if no special cause interrupts us, we shall in a few months have brought everything to the most forward condition in which it is practicable to keep it.

"For the rest, the general policy of the Astronomical Observatory has been the same as during the last twenty years; to leave the equatoreal and scrutinizing departments of Astronomy to other Observatories, but to spare no expense in instruments, no pains in observation, and no labour in reduction, on standard meridional observations generally, and on meridional and extrameridional observations of the moon in particular. And I would fain hope that the *Greenwich Observations* have assumed such a shape, that the Astronomer who may desire to find fundamental determinations of the sun's path, of the places of an extensive catalogue of stars, and of the varying positions of all the moving bodies of the solar system, presented in the utmost extent and fulness, and accuracy, may meet with all that he desires in those volumes."

There were exhibited at the meeting of the Society two beautiful engravings,—one representing two views of *Mars* and the other a view of *Saturn*,—executed from drawings by Captain Jacob, founded upon his own observations of those planets at Madras with the Lerebours equatoreal. One of the views of *Mars* represents the aspect of the planet on the 18th of March,

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1854, at $9^h\ 30^m$ Madras Mean Time; the other represents its appearance on the 23d of March, 1854, at $6^h\ 54^m$. The drawing of *Saturn* refers to the appearance of the planet on the 15th of November, 1852. The transparency of the obscure ring which was first remarked by Captain Jacob and Mr. Lassell, independently of each other, is very clearly exhibited in this drawing. The drawings of both planets have been engraved at the expense of the Court of Directors of the East India Company, who have liberally distributed copies of them to the Fellows of the Society and other persons interested in the subject of such delineations. A few copies still remain for distribution, which may be obtained by applying at the Apartments of the Society.

Suggestions respecting the Origin of the Rotatory Movements of the Celestial Bodies and the Spiral Forms of the Nebulæ as seen in Lord Rosse's Telescopes. By James Nasmyth, Esq.

"What first set me thinking on this subject was the endeavour to get at the reason why water in a basin acquires a rotatory motion when a portion of it is allowed to escape through a hole in the bottom. Every well-trained philosophical judgment is accustomed to observe illustrations of the most sublime phenomena of creation in the most minute and familiar operations of the Creator's laws, one of the most characteristic features of which consists in the absolute and wonderful integrity maintained in their action whatsoever be the range as to magnitude or distance of the objects on which they operate.

"For instance, the minute particles of dew which whiten the glass-blade in early morn are, in all probability, moulded into spheres by the identical law which gives to the mighty sun its globular form!

"Let us pass from the rotation of water in a basin to the consideration of the particles of a nebulous mass just summoned into existence by the fiat of the Creator,—the law of gravitation coexisting.

"The first moment of the existence of such a nebulous mass would be inaugurated by the election of a centre of gravity, and, instantly after, every particle throughout the entire mass of such nebulæ would tend to and converge towards that centre of gravity.

"Now let us consider what would be the result of this. It appears to me that the inevitable consequence of the convergence of the particles towards the centre of gravity of such a nebulous mass would not only result in the formation of a nucleus, but by reason of the physical impossibility that all the converging particles should arrive at the focus of convergence in directions perfectly radial and diametrically opposite to each other, however slight the degree of deviation from the absolute diametrically opposite direction in which the converging particles coalesce at

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the focus of attraction, a twisting action would result, and rotation ensue, which, once engendered, be its intensity ever so slight, from that instant forward the nucleus would continue to revolve, and all the particles which its attraction would cause to coalesce with it, would do so in directions tangential to its surface, and not diametrically towards its centre. In due course of time the entire of the remaining nebulous mass would become affected with rotation from the more rapidly moving centre, and would assume what appears to me to be their inherent normal condition, namely, spirality, as the prevailing character of their structure; and as that is *actually* the aspect which may be said to characterise the majority of those marvellous nebulæ, as revealed to us by Lord Rosse's magnificent telescope, I am strongly impressed with the conviction that such reasons as I have assigned have been the cause of their spiral aspect and arrangement.

"And by following up the same train of reasoning, it appears to me that we may catch a glimpse of the primeval cause of the rotation of every body throughout the regions of space, whether they be nebulæ, stars, double stars, or planetary systems.

"The primary cause of rotation which I have endeavoured to describe in the preceding remarks is essentially cosmical, and is the direct and immediate offspring of the action of gravitation on matter in a diffused, nebulous, and, as such, highly mobile condition.

"It will be obvious that in the case of a nebulous mass, whose matter is unequally distributed, that in such a case several sub-centres of gravity would be elected, that is to say, each patch of nebulous matter would have its own centre of gravity; but these in their turn subordinate to that of the common centre of gravity of the whole system, about which all such outlaying parts would revolve. Each of the portions above alluded to would either be attracted by the superior mass, and pass in towards it as a *wisp* of nebulous matter, or else establish perfect individual and distinct rotation within itself, and finally revolve about the great common centre of gravity of the whole.

"Bearing this in mind, and referring to some of the figures of the marvellous spiral nebulæ which Lord Rosse's telescope has revealed to us, I shall now bring these suggestions to a conclusion. I have avoided expanding them to the extent I feel the subject to be worthy and capable of; but I trust such as I have offered will be sufficient to convey a pretty clear idea of my views on this sublime subject, which I trust may receive the careful consideration its nature entitles it to. Let any one carefully reflect on the reason why water assumes a rotatory motion when a portion of it is permitted to escape from an aperture in the bottom of the circular vessel containing it; if they will do so in the right spirit, I am fain to think they will arrive at the same conclusion as the contemplation of this familiar phenomenon has brought me to.

"*Bridgewater Foundry, June* 7, 1855."

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On Celestial Day Observations. By Thomas Dick, Esq. LL.D.

"During the last nine or ten years more discoveries have been made in the heavens than at any previous period. More than thirty planetary bodies, in addition to those formerly known, have been descried by the unwearied observations of European astronomers. It has frequently occurred to me as not altogether improbable that some new planets might be discovered in the *daytime* in the region of the heavens which lies between the orbit of *Venus* and the sun.

"About the year 1813 I commenced a series of observations on the heavenly bodies in the daytime with a small equatoreal telescope, 20 inches focal length, furnished with powers of 15, 30, 45, and 100 times, with the view of determining the following particulars:—1. What stars and planets may be conveniently seen in the daytime, when the sun is above the horizon; 2. What degrees of magnifying power are requisite for distinguishing them; 3. How near their conjunction with the sun they may be seen; and, 4. Whether the diminution of the aperture of the object-glass of the telescope or the increase of magnifying power conduces most to render a star or a planet visible in daylight. Several hundreds of observations were made in reference to these particulars, and the results were published in vol. xxxvi. of Nicolson's *Philosophical Journal*, for 1813.

"Now, it occurs to me that if a series of observations were made in the daytime on that region of the heavens which lies between the orbit of *Mercury* and the sun, it might be ascertained whether any planetary bodies exist within the orbit of that planet,—a space 37 millions of miles in extent between the orbit of *Mercury* and the central point of the solar system. But such a body could never be detected in the evening after sunset, as its greatest elongation from the sun could not be supposed to be more than 8 or 10 degrees, and consequently it would descend below the horizon in less than half-an-hour after sunset, and before twilight had disappeared. The only chance of detecting such a planet would be when it happened to transit the sun's disk; but as this would only happen at distant intervals, and as it might make the transit in cloudy weather, or when the sun is absent from our hemisphere, there is little prospect of our discovering such a body in this way.

"I am of opinion that it is possible, and not at all improbable, that a planet within the orbit of *Mercury* (if such a body exist) might be detected in the daytime, were powerful telescopes applied to a space of the heavens about 12 or 14 degrees around the sun. Small stars have been seen even at noonday with powerful instruments, and consequently a planet even smaller than *Mercury* might be perceived in the daytime. On the 2d of October, 1843, about 2 o'clock, P.M., I perceived the planet *Venus*, about 2 hours before her superior conjunction with the sun, when only 58′ from the sun's northern limb.

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"In making such observations, it is requisite that a round opaque body be placed at a considerable distance from the observer, so as completely to intercept the body of the sun, and about $1\frac{1}{2}^{\circ}$ of the heavens all around him; and every portion of the surrounding space, extending to at least 12 or 14 degrees in every direction, should then be carefully and frequently examined by good telescopes. The opaque circular board, or other object, in order effectually to intercept the rays of the sun, should be placed at a considerable distance beyond the object-end of the telescope, and and if any contrivance could be made to make it move along with the sun so much the better, so as to prevent any of the solar rays from entering the tube of the telescope. It would likewise be expedient that the object-glass of the telescope should be somewhat contracted. Such observations, if persevered in, would undoubtedly afford a chance of detecting any revolving body that might exist within such a limit. It is not at all improbable that a planet exists within the range of 36 millions of miles from the sun; and from many observations I have made on *Venus* when very near the sun I am persuaded that a body, though only half the size of *Mercury*, could easily be distinguished in daylight with a good telescope.

"*Broughty Ferry, Dundee, May* 31, 1855."

The King of Prussia has presented a Gold Medal to Lieut. Maury, U.S.N., on account of "the distinguished services which he has rendered to science and navigation by his labours in ascertaining the currents and depths of the ocean, and in determining the direction of the winds at different seasons and in different latitudes." This mark of distinction was also accompanied with the presentation of one of the gold medals struck in honour of the publication of Baron Humboldt's *Cosmos*.

The ephemeris of *Amphitrite* for 1855, published in the supplement to the *Nautical Almanac* for 1858, having been found to exhibit a large difference from observation, Mr. Hind has issued a printed circular containing a portion of M. Villarceau's ephemeris of the planet (*Comptes Rendus*, tom. xl. No. 5), adapted to the meridian of Greenwich. This reprint contains the apparent places of the planet from July 1 to September 30 of the current year.

Remarks on the Construction of Telescopes with Simple Cylindrical Glasses. By M. Sturm.

(*Communicated by Dr. Lee.*)

"I take the liberty of calling attention to the application of cylindrical lenses to the construction of telescopes, which are,

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as nearly as is possible, free from the defects of aberration: for the special purpose of grinding and centering these glasses I have constructed and patented a machine, and the glasses so ground and centered appear more perfect than those at present manufactured by hand. The defectiveness of the cylindrical glasses now in use is, I think, the reason why they have not been hitherto more used for scientific purposes. I have made a telescope, by way of experiment, with a cylindrical object-glass ground on my machine; the effect of which sufficiently shows the practicability of my method. Although I have at present but one cylinder in my grinding machine, it is, I think, demonstrable, that when I am enabled to construct the several cylinders of the different focal lengths and diameters, important advantages may accrue to the science; among which might be mentioned,—

"Firstly. Having the object-glass single, thus obviating the necessity of different kinds of glass, and the difficulty of procuring flint-glass of a large size free from imperfections.

"Secondly. Object-glasses could be produced of a diameter of from 20 to 24 inches.

"Thirdly. The magnifying power and the extent of field will be proportionately increased, and with equal precision and exactness.

"Fourthly. The time required for manufacturing such object-glasses is considerably less; and

"Fifthly. The cost is greatly diminished."

It may be as well to remind those who possess telescopes of sufficient optical power for the purpose, that it would be desirable to examine the planet *Venus* on the occasion of its approaching inferior conjunction, with the view of ascertaining whether it affords any indications of a twilight conformably to what has been asserted by some preceding observers. (See *Monthly Notices*, vol. xiv. p. 169.)

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XV. *Supplemental Notice.* No. 9.

Death of the Rev. Richard Sheepshanks.

It is with deep regret that the Editor has to announce the heavy loss which the Society has sustained during the recess, in the death of one of its most valuable and devoted adherents. On the 4th of August, 1855, the Rev. Richard Sheepshanks, M.A., after a short illness, expired at his residence in Reading, aged sixty-one years. This is not the occasion on which to advert at length to the admirable qualities of heart which endeared Mr. Sheepshanks to a wide circle of friends; nor to remark upon the disinterestedness with which he laboured throughout life in the cause of science, and more especially upon his untiring vigilance in promoting the interests of this Society during the long period of his connexion with it. All this will be recorded in the proper place. But the Editor may be permitted to state, that in the death of Mr. Sheepshanks he has to deplore the loss of a warmly-attached friend, whose valuable counsel and effective aid in discharging the duties of his office were invariably tendered with a cordiality and kindliness of feeling, the vivid recollection of which must constitute his apology for alluding to them here.

Observations of the Comet discovered at the Imperial Observatory at Paris on the 4th of June (Comet II., 1855), taken with the 8½-inch Achromatic Refractor of the Liverpool Observatory. By John Hartnup, Esq.

G.M.T.	Comet's R.A.	Log. $\frac{p}{P}$	N.P.D.	Log. $\frac{q}{P}$
1855. d h m s	h m s		° ′ ″	
June 9 11 27 3·5	7 51 51·70	+8·582	54 45 35·6	−9·9380
10 10 39 59·4	7 58 40·26	+8·641	55 3 0·6	−9·9057
10 11 18 6·1	7 58 51·01	+8·597	55 3 26·6	−9·9313

The observations are corrected for refraction. The corrections to be applied for parallax in time and arc are represented by p and q. P is the equatoreal horizontal parallax; star of comparison, B.A.C. 2860; assumed mean place for 1855, Jan. 0.

$8^h\ 24^m\ 3^s{\cdot}09$ R.A. $53^\circ\ 4'\ 32''{\cdot}65$ N.P.D.

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Observations made in 1851, *to determine, by means of the Electric Telegraph, the Difference of Longitude between Halifax Dock-yard Observatory and Harvard Observatory, Cambridge, Massachussets, U.S.*

(*Communicated by the Astronomer Royal.*)

The observatory in the Naval Yard at Halifax was placed in communication with the observatory at Bangor (Maine), and at Cambridge (Mass.) by means of the electric telegraph.

On the 25th of November, 1851, the break-circuit clock was put in operation at Bangor, and compared with a sidereal chronometer at Halifax, but the weather was too cloudy for observations, and continued so until the 16th of December.

The instruments used were four-feet portable transits, by Troughton and Simms, belonging to the United States Coast Survey, having each twenty-five vertical wires arranged in five groups of five wires each, and one horizontal wire through the centre, at the principal focus of the object-glass.

The time was defined by the clock at Bangor breaking circuit every second, which, together with the observations, were recorded on the registers at each place.

The following are the results of observations made on the 16th December, 1851:—

By Bangor Clock.

	h m s
B.A.C. 1287, passed M^n. Halifax, lamp W.	3 43 36·84
— — Camb. — E.	4 13 46·47
Halifax, east of Camb.	30 9·63
B.A.C. 1736, passed M^n. Halifax, lamp E.	5 4 39·87
— — Camb. — W.	5 34 49·49
Halifax, east of Cambridge	30 9·62
Mean free from collimation error	30 9·625

	m s
Bangor clock slow of Halifax by 1414, lamp W. (12 wires) ...	20 26·44
— — — 1476, — (25 wires) ...	26·15
Meaning half effect to No. of wires	20 26·27
Bangor clock slow of Halifax by 1540, lamp E. (25 wires) ...	20 26·60
Meaning lamp E. with lamp W., frees collimation	20 26·435

	m s
Bangor clock fast of Cambridge by 1364, lamp E. (25 wires) ...	9 43·31
— — — 2429, — — ...	43·54
— — — 2504, — — ...	43·17
Mean, lamp E.	9 43·34

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	m s
Bangor clock fast of Cambridge by 2338, lamp W. (25 wires) ...	9 42·79
— — — 2691, — — ...	9 43·21
Mean, lamp W.	9 43·00
Meaning lamp E. with lamp W. frees collimation	9 43·17
	m s
Hence Bangor clock slow of Halifax	20 26·435
— — fast of Cambridge	9 43·170
Halifax, east of Cambridge	30 9·605

By direct observations on B.A.C. 1287 and 1736, Halifax Naval Yard Observatory is east of Cambridge (Harvard Observatory), $30^m\ 9^s{\cdot}625$. By indirect observations, $30^m\ 9^s{\cdot}605$. Taken, $30^m\ 9^s{\cdot}62$.

The transit instrument was 94·7 feet west of Cambridge east transit, that to which the longitude of the observatory is referred.

Therefore Halifax Naval Yard Observatory is $30^m\ 9^s{\cdot}54$ east of Cambridge (Harvard Observatory, east transit).

At this point Professor Bond was taken dangerously ill, and we were unable in consequence to correct again with Cambridge.

Observations were made on the 17th, 19th, 22d, and 23d December, between Halifax and Bangor. The weather was generally unfavourable, and also the state of the telegraph; however, the observations tended to confirm the above result, which cannot be far from the truth.

The mean distance determined by Admiral Owen during 1841, 1844,* and 1845, with chronometers, in which I assisted, was $30^m\ 9^s{\cdot}9$.

	h m s
Harvard East Transit being taken West of Greenwich	4 44 30·54
Halifax, east of Harvard (east transit)	30 9·54
Halifax Observatory west of Greenwich	4 14 21

P. FREDERICK SHORTLAND,
Commander.

Elements of Comet I., 1855. By M. Winnecke.†

T	1855, Feb. 5, 76590.	
π	226 33 4·6	Equinox 1855, Jan. 0.
Ω	189 40 8·6	
i	51 12 41·2	
Log q ...	0·341478	

Motion retrograde.

These elements are calculated from the Moscow observation of April 19, and the Berlin observations of May 6 and May 18.

* In the determination of the longitude of Harvard Observatory the greatest weight has been given to the result of numerous chronometers run between Liverpool Observatory and that at Harvard, by means of the Cunard steamers, in which Liverpool Observatory is taken as $12^m\ 0^s{\cdot}5$ west of Greenwich.

† Discovered by M. Schweizer on the 11th of April; see p. 174, where for Comet II., read Comet I.—EDITOR.

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Discovery of a New Planet. By M. Luther.

Mr. Hind has received a letter from M. Luther, dated October 8, announcing his discovery of a new planet at Bilk, on the 5th of the same month. It was situate in 2° 25′ of right ascension, and 0° 52′ of north declination, and resembled a star of the tenth magnitude. On the following evening he obtained this observation of it:—

	M.T. Berlin.	R.A.	N. Decl.
1855.	h m s	° ′ ″	° ′ ″
Oct. 6.	8 44 14·4	2 12 21·4	+0 49 18·2

This planet has received the designation of *Fides*, with the symbol of a cross.

On certain Anomalies presented by the Binary Star 70 Ophiuchi. By Capt. W. S. Jacob, Madras Astronomer.

The pair of stars designated as 70 *Ophiuchi* have been long recognised by astronomers as a binary system, but the authorities are not as yet agreed as to the exact orbit, although only about 20° are wanting to complete the whole ellipse since they were first measured by Sir W. Herschel in 1779.

Many hypothetical orbits have been computed, all of which fail at certain points in representing the observed quantities as closely as might be expected: thus, in some that have lately appeared, while the angles show a tolerably near agreement with observation, the distances are entirely thrown out, the maximum being represented as yet future; whereas, observation indicates a steady decrease in distance since 1848 or 1850, the quantity now amounting to 0″·50; any attempt, however, to alter the elements so as to bring the distances into better agreement, throws a considerable increase of error on the angles. There is, further, this remarkable point to be noticed, viz., that even in those orbits in which the distances are neglected, and the angles made to agree as closely as possible, the errors assume somewhat of a periodical or epicyclical form, continuing to have the same sign over a considerable extent of the orbit, and this is still more strongly marked when both angles and distances are taken into account. Thus, in an orbit which I have lately computed with a period of 93 years, the errors are + from 1820 to 1823, — with one exception from 1823 to 1830; from 1830 to 1832 there is a turning point, where the errors alternate; and from 1833 to 1842, they are again +; and from 1846 to the present time they are for the most part —.

Now such systematic errors can hardly be casual; they must

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depend upon some law. It is just possible that they might arise from an erroneous assumption of the universal application of the law of gravitation; this law may be somewhat modified in these remote systems: the general evidence of observation is in favour of the law being everywhere the same as it is within the solar system, but the data are not exact enough to define this with certainty.

But there is a simpler mode of accounting for the discrepancy without having recourse to any change of law. We may suppose a third body to belong to the system, and to be opaque, and consequently invisible; such a body would, of course, disturb the regularity of the motions of the other two.

The existence of such dark bodies has been already surmised, though not fully demonstrated in the cases of certain apparently single stars, such as *Sirius* and *Procyon*. The body in this case, if it be supposed to circulate as a planet round the smaller star, need not be very large, for the observed amount of deviation of the star from its regular orbit is less than 0″·1 of arc.

I have computed the corrections to be applied on the hypothesis of the secondary orbit described by the smaller star having a semi-major axis of 0″·08, and an excentricity of 0″·15, a periodic time of 26 years, and motion direct, which seems to be somewhat near the truth, for a great improvement is at once manifested; without correction the average error of the angles in the interval from 1820 to 1855 is 49′, which is reduced by the corrections to 35′, or by more than one-fourth, while the maximum error is also reduced from 133′ to 94′. In the distance measures for the same interval the difference is less strongly marked, but the early measures are not worthy of much confidence; and if we consider only the time subsequent to 1838, which includes all the best measures, we find the average error reduced from 0″·14 to 0″·11, and the maximum from 0″·26 to 0″·19, being about the same proportion as above.

There is, then, some positive evidence in favour of the existence of a planetary body in connexion with this system, enough for us to pronounce it highly probable, and certainly good ground for watching the pair closely, to procure, if possible, still stronger evidence.

The corrections have not been applied to the three first observations, being those of Sir W. Herschel; these being isolated points, could be easily brought into agreement by a slight change of the elements, and would, therefore, furnish no test of the accuracy of the hypothesis.

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Comparison of the Orbit of 70 Ophiuchi, with Observations.

Epoch.	Observed Position Angles.	Computed — Observed.	Error Corrected.*	Observed Distance.	Computed Distance.	Error.	Error Corrected.*
	° ′	′		″	″		
1779·77	90 0	+ 5	...	4·49	4·67		
1802·34	336 8	−136	...				
1804·41	318 48	+ 12	...	2·56	2·44		
1820·31	161 27	+133	+65				
1821·51	156 50	+107	+67				
1822·54	154 30	+ 25	+14				
1823·32	153 25	− 50	−41				
1825·56	148 12	− 92	−44	4·38	4·29		
1826·75	146 24	−133	−80				
1827·40	143 54	− 64	−10	4·51	4·64		
1828·67	140 18	+ 13	+67	4·79	4·87		
1829·50	139 30	− 23	+23	5·18	5·00		
1830·36	138 9	− 22	+ 8				
1830·50	137 28	+ 6	+35	5·65	5·20		
1830·76	136 24	+ 46	+71	5·43	5·24		
1831·55	136 8	− 5	+ 8	5·97	5·37		
1832·55	133 46	+ 53	+53	5·73	5·52		
1832·57	135 31	− 54	−54	5·50	5·52		
1833·42	132 49	+ 43	+33	6·14	5·65		
1833·59	132 30	+ 49	+37	5·98	5·68		
1835·56	130 36	+ 17	−13	5·97	5·91		
1836·81	128 36	+ 52	+12	6·33	6·05		
1837·64	127 30	+ 63	+19	6·40	6·14	″	″
1838·51	126 30	+ 67	+20	6·25	6·22	−·03	−·05
1842·55	122 24	+ 67	+27	6·68	6·57	−·11	−·06
1846·21	120 10	− 2	− 2	6·83	6·64	−·19	−·11
1848·12	118 50	− 27	−10	6·80	6·66	−·14	−·06
1850·48	115 11	+ 64	+94	6·86	6·65	−·21	−·17
·66	117 0	− 55	−23	6·50	6·64	+·14	+·16
1852·75	114 3	+ 5	+40	6·73	6·61	−·12	−·13
1853·60	114 39	− 77	−46	6·49	6·58	+·09	+·05
1854·08	113 39	− 44	−16	6·36	6·57	+·21	+·16
1854·24	113 2	− 16	+10	6·51	6·57	+·06	+·01
1854·73	113 43	− 84	−62	6·34	6·55	+·21	+·16
1855·45	111 35	+ 4	+21	6·26	6·52	+·26	+·19
		—	—			—	—
		49	35			·14	·11

Elements of the Orbit of 70 Ophiuchi.

τ = 1808ʸ·12
ϖ = 292° 32′
Ω = 304·32
λ = −20 28
e = ·4894
γ = 55° 16′ cos = [9·7557]
P = 93ʸ·10
n = 3°·867

* On the hypothesis of a secondary orbit, in which a = 0″·08, e = 0·15. ϖ = 200°, P = 26ʸʳ, τ = 1825·5.

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Elements of Comet II., 1855. By M. Bruhns.*

T	1855, May 29, 23896, M. T. Berlin.	
π	240 15 18·4	Mean Equinox 1855, 0.
Ω	260 52 43·1	
i	22 58 27·1	
Log q ...	9·745678	

Motion retrograde.

These elements were calculated from the Berlin observations of June 5, 6, and 7.

Discovery of a New Planet. By M. Goldschmidt.

On the 5th of October M. Goldschmidt discovered a new planet at Paris; it resembled a star of the 11–12th magnitude. The following observation of its right ascension was taken at the Imperial Observatory, Paris:—

1855.	M.T. Paris. h m s	R.A. h m s
Oct. 7	9 55 37·9	22 59 31·86

The declination is wanting.†

This planet has received the name of *Atalanta*.

Observations of the two New Planets discovered by M. Goldschmidt and M. Luther on October 5, 1855, made with the Hamburg Equatoreal. By Mr. G. Rümker.

(*Communicated by Dr. Lee.*)

M. Goldschmidt's New Planet (*Atalanta*).

1855.	Hamb. M.T. h m s	App. R.A. ° ′ ″	Decl. ° ′ ″	App. Place of Star of Comp. h m s	° ′ ″
Oct. 12	10 13 53	343 50 38·7	−6 48 42·3 (5 comp.)	22 56 59·44	−6 57 19·3
13	8 4 36	343 40 42·6	−6 40 34·5 (18 comp.)	22 55 33·91	−6 36 53·8

M. Luther's New Planet (*Fides*).

1855.	Hamb. M.T. h m s	App. R.A. ° ′ ″	Decl. ° ′ ″	App. Place of Star of Comp. h m s	° ′ ″
Oct. 7	7 50 57	2 0 3·3	+0 45 49·0 (17 comp.)	0 6 25 21	+0 34 59·3
8	7 46 54	1 47 14·9	+0 41 55·1 (15 comp.)	0 6 25 21	+0 34 59·3
				0 7 14 co	+0 29 45·9
13	10 10 13	0 44 57·0	+0 23 59·4 (19 comp.)	0 2 4 94	0 26 30·3

* This is the comet alluded to at p. 203, as having been discovered by M. Klinkerfues on the 4th of June. It was also discovered independently on the same evening at the Imperial Observatory of Paris, by M. Dien, and on the *preceding* evening by Dr. Donati at Florence.—Editor.

† It appears to be about $-6\frac{1}{2}°$, see *infrà*.—Editor.

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Supplement to the Nautical Almanac for the year 1859.

This Supplement (which has just been published previous to the appearance of the Almanac itself) contains Ephemerides for the year 1856, adapted to the meridian of Greenwich, of *Ceres, Pallas, Juno,* and *Vesta;* approximate at intervals of four days, and accurate at each transit near the times of their respective oppositions; with the elements from which they have been deduced. Also, approximate Ephemerides of all the newly-discovered Planets, with the exception of *Circe, Leucothea,* and the two just announced, one by Dr. Luther, and the other by Mr. Goldschmidt; with elements in a few special cases of addition of perturbations.

It may be proper to remark with respect to *Ceres* and *Juno,* that the necessity for a correction of their elements has been for some time apparent; but that as regards *Pallas* and *Vesta,* the case is less urgent, hence the reason for allowing the two latter to continue uncorrected for the present.

A letter has been received from Mr. Mayne, a gentleman residing in Killaloe, county of Clare, Ireland, in which he states, with reference to an extract of a letter from M. Sturm on the construction of cylindrical object-glasses, which was communicated to the Society by Dr. Lee, and inserted in the *Monthly Notices* for last June, that he purchased glasses of a similar construction in Paris as early as the year 1814. Mr. Mayne says nothing respecting the *practical value* of those glasses as regards their achromatic and other qualities; he, no doubt, presumes that any information on this point would be unnecessary. The Editor avails himself of this opportunity of repeating what has been already announced on several previous occasions — that the Society distinctly disavows any responsibility in connexion with the opinions expressed in any of the papers which appear in its publications.

The Editor is indebted to the Astronomer Royal for pointing out (in a letter dated Aug. 11, 1855) an oversight committed in a note inserted in the *Monthly Notices* for last June (p. 224), relative to the expediency of observing *Venus* on the occasion of her approaching inferior conjunction in October. "There is no possibility," says Mr. Airy, "of seeing *Venus* near this conjunction: 1st, because, even if she were in the ecliptic, the ecliptic itself is so nearly parallel to the horizon, that nothing can be seen within 30° of the sun (this, however, does not apply to the rising); 2dly, because she has a large southern latitude. The only conjunctions when *Venus* can be advantageously seen are those in the spring."

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Page 12, line 8 from bottom, *for* a large number, *read* a moderate number.
— 14, — 1, *for* were different, *read* were not different.
— — last line, *for* greater than unity, *read* less than unity.
— 49, line 2 from top, *for* Lieut. G. M. Gilliss, *read* Lieut. J. M. Gilliss.
— 159, — 11, *for* Comet I., 1855, *read* Comet V., 1854.
— 161, — 3 from bottom, *for* agreement, *read* argument.
— 174, — 10, *for* Comet II., 1855, *read* Comet I., 1855.
— 179, the upper line of Fig. X should be slightly curvilinear, so as to adapt itself to the periphery of the elliptical orbit of the star.
— 181, line 4 from bottom, *for* 52° 24′ 52″, *read* 52° 54′ 52″.

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